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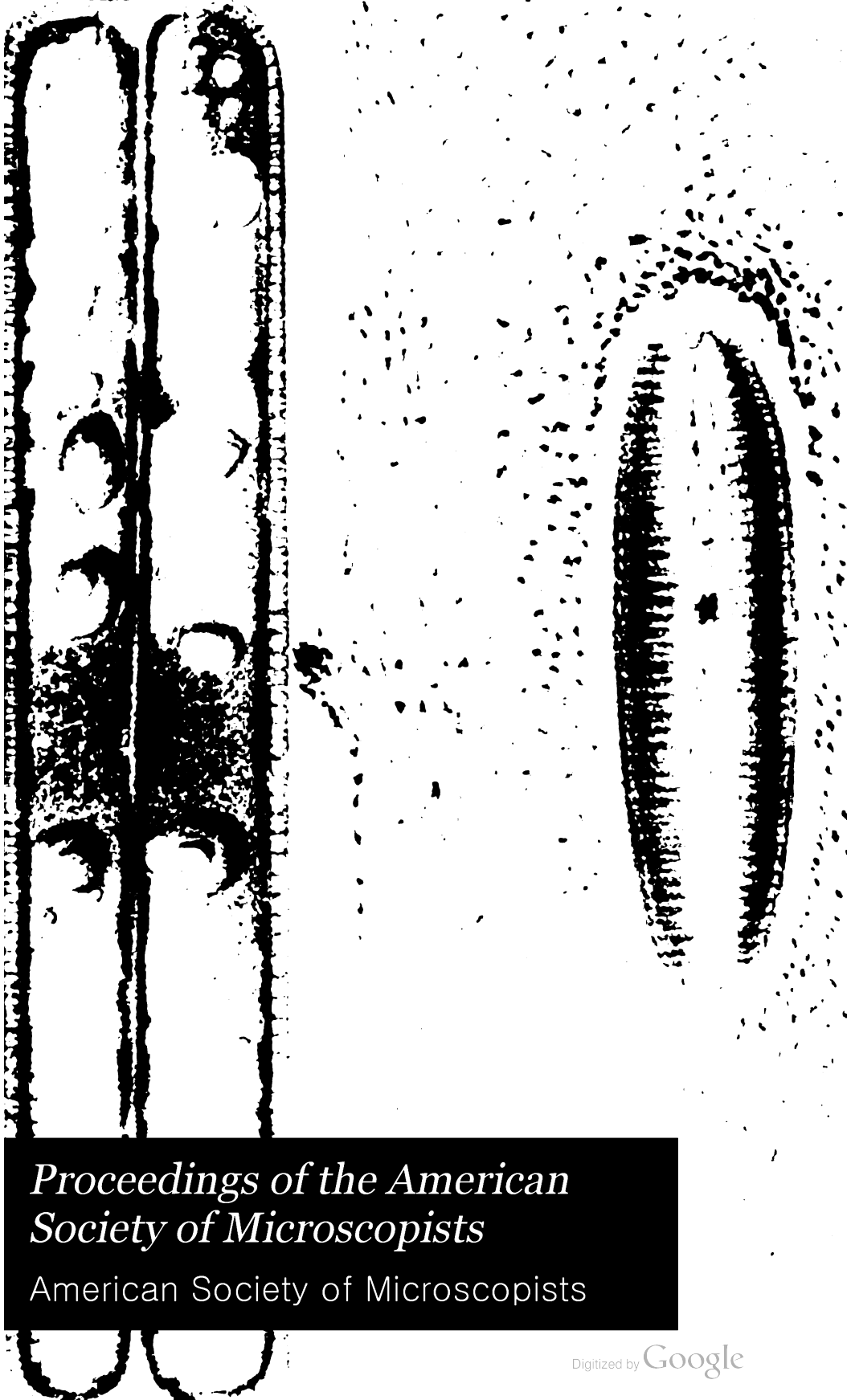

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PROCEEDINGS

OF THE

AMERICAN SOCIETY

OF MICROSCOPISTS

TENTH ANNUAL MEETING

HELD AT

PITTSBURGH, PA.

AUG. 30TH AND 31ST, AND SEPT. 1ST AND 2D, 1887.

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PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MICROSCOPISTS

TENTH ANNUAL MEETING.

ANNUAL ADDRESS OF THE PRESIDENT:

THE MICROSCOPE AS A FACTOR IN A STUDY OF THE BEHAVIOR OF METALS UNDER VARIATIONS OF TEMPERATURE.

WILLIAM A. ROGERS, A. M., Hon. F. R. M. S., Waterville, Maine.

Microscopy is a cosmopolitan science. We may go farther than this and say that microscopy is more nearly cosmopolitan in its character than any other science. If I did not believe this to be true, I should not have consented to occupy the honorable position which I now hold by your suffrages, for there are many members of this society to whom the honor more justly belongs by virtue of greater familiarity with the technics of our science. I suppose that I am indebted to this expression of your confidence on account of the use which I have made of the microscope as an essential factor in a single line of research.

It is the glory of our science that the microscope supplements the natural vision to such an extent that we can submit nearly every theory, nearly every deduction from experiment, nearly every fact of observation, to the supreme and only test by which a real truth in nature can be estab-

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lished, viz.: through the medium of one of the senses with which we have been endowed by the Creator. It has been said that microscopy has no claim to be regarded as a science, and that the microscope is simply an instrumental agent occupying, with respect to other sciences, a position similar to that which the telescope sustains in its relation to astronomy. A convincing answer to this criticism is found in the fact that the telescope is limited in its application to a comparatively narrow field of research. Where the telescope answers a single question the microscope answers a thousand. Spectroscopy has become a recognized science, not so much because of its revelations in regard to the nature of light, as on account of the application of the spectroscope as an instrument, to the study of the physical properties of matter and of motion not only on the earth but in worlds other than our own.

In discussing the question whether microscopy can be regarded as a science, we must always bear in mind the fact that a science is only a convenient name *for a group of similar laws of nature*, and that the term is properly applicable not only to the development of these laws, but to their application to the useful economies of life. Thus we have the science of engineering, in which mathematical analysis is as much an essential part as skill in mechanical construction. But this analysis would serve no useful purpose if it did not rest ultimately on facts of observation.

The limitations which necessarily belong to a definition of physical science are clearly expressed by Tate in his most admirable treatise on Heat. He says: "Nothing can be learned as to the physical world save by observation and experiment, *or by mathematical deductions from data so obtained.*" Now the microscope as an instrument of research stands unrivaled, not only in respect to the precision of the observations made with its aid, but also in the universality of its application in furnishing what Tate calls "the data so obtained."

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Each succeeding year witnesses an extension of the range of its applications. Within a few years, while retaining its claim as an essential factor in scientific research, it has also become a very material aid in many mechanical industries. It is a common impression that the microscope is too delicate an instrument to be used in the ordinary operations of mechanical construction, and that the apparent necessity of using transmitted light for the purpose of illumination is an absolute barrier to any extended employment of the instrument. The latter difficulty is entirely obviated by the use of the opaque illuminator, invented by Tolles, by which a bright metal surface can be examined with the utmost ease, while actual experience has shown that it is by no means necessary that the instrument should be mounted upon massive piers insulated from surrounding objects.

I cannot more forcibly combat this impression than by referring to two cases within my own experience. The Proceedings of the Society of Mechanical Engineers for 1884, contains a description of a method of cutting a screw in which each thread is made to correspond in pitch with equal subdivisions of a standard yard traced upon a metal bar. The screw for the engine constructed for the Cornell University was made in this manner. Professor Anthony has shown that the maximum accumulated error of the screw does not reach two mikrons for a limit of twenty inches, while the actual error at any selected point does not reach one mikron. This screw was cut in the manner indicated, in the third story of a building occupied by machinery which produced a decided tremor in every room. It was only found necessary to make the attachment of the microscope to the compound rest of the lathe very firm, and to brace the bed of the lathe very securely from the floor.

The writer was recently called upon to "level up" the bed of a very heavy planer, having ways eighteen feet in length. Several days had been spent in securing as good an adjustment as could be obtained with the aid of a spirit

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level of special construction. A plank twenty-two feet in length, eight inches in width, and two inches in thickness, was set up edgewise beside the platen of the planer, but insulated from it. A groove half an inch wide and half an inch deep was ploughed in the upper face of the plank, and after having stopped both ends, the groove was filled with mercury. The surface of the mercury then formed an invariable plane of reference. The microscope was securely attached to the platen, and adjusted for a sharp focus upon the surface of the mercury at one end. The platen was then moved along until the microscope occupied a position near the other end of the groove. This end was then adjusted by elevation or depression, as required, until the surface of the mercury was sharply in focus. After two trials it was found that the surface of the mercury was at the same constant focal distance from the microscope as indicated by the sharpness of definition. Notwithstanding the fact that extreme care had been taken in the original adjustment by the aid of the spirit level, it was found that as the platen moved toward the central part of the bed, the focus became more and more indistinct, indicating that the central part was too low. The proper elevation was then made at these points by means of heavy set-screws, when it was found that the mercury was sharply in focus under the objective throughout the entire range of motion. As a check upon the accuracy of the adjustment a surface plate, eight feet in length, was now planed, when it was found that the deviation from a true surface did not at any point, exceed the third part of the thickness of tissue paper. Two facts of considerable importance are to be noticed in connection with this experiment. First, that the time occupied for the complete adjustment was only twenty-five minutes; and, second, that during the entire operation the machinery of the shop was running at half speed.

These and similar observations have led the writer to advocate a more extended use of the microscope in the

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every-day work of the machine shop. By attaching the microscope firmly to the slide rest of the lathe, the ordinary operations of turning shoulders to a given length, and of cylinders to a given diameter can be more expeditiously, more exactly and more economically performed than by the usual method.

It is freely admitted by mechanicians that a decided advance in mechanical constuction would be made by the employment of uniform measures of length. This can be easily and profitably accomplished in any well regulated shop, employing as many as fifty hands, by delivering from a standards-room any desired unit of length, in the same way that tools are delivered from a tool-room. The expense of a comparator, from which any measure of length could be obtained within a limit of time which would not ordinarily exceed one minute, would not be great. If this comparator were placed in charge of a person familiar with its use, and in a convenient location, any workman could have a calliper set for him in half the time that would be required in setting it to a scale by the usual method; the precision would be incomparably greater, and absolute uniformity would be secured in every dimension of length employed. The various points to which I have briefly called attention, are to be considered simply as illustrations of the many ways in which the useful service of the microscope may be extended.

In the address which I am called upon to make this evening, as President of the American Society of Microscopists, I have selected a single application of the microscope in scientific research. *I beg to call your attention to the microscope as a factor in a study of the behavior of metals under variations of temperature.*

If a bar of metal which has the faces of each end parallel and at right angles to its axis, is submerged in melting ice, the perpendicular distance between the two faces may be said to represent a definite unit of length at the temperature of 32° F. or 0° C. If this distance is identical in

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length under similar conditions, with a certain bar of platinum now deposited at the International Bureau of weights and measures at Breteuil, near Paris, and designated the "*Metre des Archives*," the length of the bar is said to be one meter. If now the bar is submerged in a liquid which has throughout its entire mass a temperature one degree higher than that of melting ice, its length, after it has reached the same temperature as the liquid, will be increased by a certain fraction of its entire length. If this length is subdivided into one million equal parts, and if the increase is, for example, ten parts in one million, the coefficient of expansion of the metal is said to be ten mikrons. If the increase in length proceeds uniformly for each and every increment of temperature, we can say, for example, that the length of the bar at 100° C. will be 1000 mikrons, or one millimeter greater than it was at 0° C. We can also say that if the temperature of the entire mass of metal is again reduced to 0°, the length of the bar will be exactly the same as it was before the increase of temperature took place.

There is some evidence that when certain metals are exposed to very violent changes in temperature, as when zinc is removed from a temperature of 100° C. and is submerged in melting ice, the molecular arrangement of the metal is disturbed to such an extent that the return to its original condition may be delayed for several days, and even for several weeks; but it cannot, at the present time, be positively asserted that the return will not ultimately take place.

It will be noticed that the definition of the coefficient of expansion which has been given, viz: the increase in length due to an increase in temperature from 0° to 1°, contains the important limitation that the entire mass of the metal shall have reached the temperature of 0°.

If the total expansion due to a rise from the temperature v to the temperature v^1 is defined to be the increase in length corresponding to a rise of 1° multiplied by the number of

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degress between v and v^1, we must understand that the change for a single degree is at every point a constant quantity.

Let a=the length of a meter at 0° C.

v=the observed temperature of the entire mass of the metal at any time, T.

b=the change in length corresponding to a change in temperature from 0° to 1°.

Δ=the length of the meter at the temperature v.

We shall then have—

$$\Delta = a + bv \qquad (1)$$

Every observed measure of length will finish an equation of the form (1).

For any other temperature b^1, the length Δ^1 will be expressed by the equation,

$$\Delta^1 = a + bv^1 \qquad (2)$$

Since a and b relate by definition only to the temperature 0°, they are therefore constant.

It will be seen that the difference in length due to a change from v to v^1 will be found by subtracting equation (1) from equation (2)

or: $$\Delta^1 - \Delta = (v^1 - v)b$$

whence $$b = \frac{\Delta^1 - \Delta}{v^1 - v} \qquad (3)$$

In order that equations (1) and (2) shall hold generally, it is necessary that the coefficient of expansion b shall be constant for the entire range of temperature between v and v^1. In other words, if the increase in length is 10 mikrons from 0° to 1°, the increase from 1° to 2° must also be 10 mikrons, in order that we may be able to say that the increase from 0° to 2° is equal to 2×10 mikrons.

It is just at this point that our inquiry begins. With few exceptions it is assumed by physicists *that the coefficient of expansion increases in value with the temperature.*

Our equation (1) would in this case bccome

$$\Delta = a + bv + \tfrac{1}{2}b^1v \qquad (4)$$

in which b^1 represents the correction to b required at the

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mean temperature $\frac{o+v}{2}$ or $\frac{1}{2}v$. Now if the law of expansion is not a direct and linear function of the temperature, equation (4) must be regarded as only an approximation to the true law, since we have assumed that b^1 is a constant for the mean temperature $\frac{1}{2}v$. Theoretically, we should have:

$$\Delta = a + bv + \tfrac{1}{2}b^1v + \tfrac{3}{4}b^2v + \tfrac{7}{8}b^3v + \tfrac{15}{16}b^4v + \&c. \quad (5)$$

in which the coefficients b^2 b^3 and b^4 hold for the temperatures $\frac{\frac{1}{2}v+v}{2}$, $\frac{\frac{3}{4}v+v}{2}$, $\frac{\frac{7}{8}v+v}{2}$, &c.

It might at first sight seem to be purely a mathematical excursion to determine whether the law of expansion is expressed by equation (1) or by equation (5); but if it is determined by equation (1) we can make use of this fact in an invesitgation of the highest practical importance, while if the coefficient of expansion is not a constant for each and for every degree of temperature considered, we become hopelesly involved in the determination of the coefficients b, b^1, b^2, b^3, b^4, &c. from a series of equations of the form (5).

Let us state as clearly as possible the problem which we have before us.

The temperature is indicated by a thermometer. If we employ a mercurial thermometer to measure the temperature, the hight of a column of mercury is indicated by a zero graduation when the temperature of the entire mass of the mercury in the bulb of the thermometer and of the glass of which the stem is made, is the same as that of melting ice. The hight of the column at the boiling point of water is marked 100° C. under similar conditions. We can now say that the readings of the scale truly represent the temperature at these two specific points. If the distance between these two points is subdivided into an equal number of parts, for example, 100, we can say of any reading of the hight of the mercury column at intermediate points, that this scale-reading represents the real temperature if two conditions are fulfilled; first, if the mercury expands uniformly for each degree of temperature, and second, if the bore of the glass

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tube which contains the mercury has the same diameter at every point. Since the latter condition can never be exactly fulfilled in the process of blowing the glass except by accident, and since the first condition introduces an additional element of doubt on account of the probable increase of the viscosity of the mercury as it approaches its freezing point, considerable doubt must always attach to the reading of a mercurial thermometer, especially at very low temperatures, even after a large part of the errors have been removed by the process of calibration. It seems to be fairly well established, that for high temperatures mercurial thermometers read too high when compared with the air thermometer, which is generally regarded as reliable in its indications. In a case cited by Sir William Thompson, the readings of three mercurial thermometers were 329.8°, 327.2°, 321.8°, respectively, corresponding to 320.0° for the air thermometer. All of the investigations made by the writer point to the conclusion that mercurial thermometers as a class, read too low when the temperature falls below 10° to 15° Fahr.

It should be said, however, that recent comparisons of several mercurial thermometers with an air thermometer at temperatures as low as minus 38° Fahr. made by the U. S. Signal Office, indicate a very close agreement. The correction of the mercurial standard Green No. 1503 at minus 28° was found to be only —0°.1. The observations of Grunmach, made in 1881, show a variation of less than 0°.1 at —20° C.

Under this state of uncertainty, it will be admitted that any independent method of measuring the temperature in which there shall be an entire freedom from the class of errors peculiar to mercurial thermometers, and to the air thermometer, would be of great value in removing existing doubts.

The method of indicating temperature by noting the relative changes in length between two bars of metal having widely different coefficients of expansion, has received con-

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siderable attention. It has been used in several trigonometrical and topographical surveys, but with results not altogether satisfactory. In addition to the assumed uncertainty with regard to the constancy of the coefficients of expansion of the two metals employed, an additional element of doubt has been introduced by joining the metals by a ligature near the center, thus introducing a compound effect due to the different rates at which the two bars arrived at complete saturation after changes of temperature occur.

The present investigation is an attempt to show that the relative expansions of Baily's metal, of Jessup's steel, and of Chance & Son's glass, are linear functions of the temperature, within the limits of the accidental errors of observation, and that these metals may therefore be safely employed to measure variations of temperature at least between —5° C. and +35° C. These measures will therefore serve as a check upon the corresponding readings of mercurial thermometers. The following example will serve as an illustration of the practical bearing of this inquiry.

It will be shown subsequently that the bronze bar of Baily's metal expands 6.20 divisions of the micrometer for each degree Fahrenheit in temperature more than the bar of Jessup's steel under the same conditions. At 62.0 degrees Fahrenheit the yard upon the steel was found to be 9.8 divisions longer than the yard upon the bronze bar. At minus 19.8 degrees the steel yard was found to be 490.5 divisions longer than the bronze yard. Hence by equation (1)
-490.5 div. $= -9.8 + [62.0 + 19.8] \times -6.20 = -517.0$ divisions.
The observed difference is 490.5 divisions while the difference due to an increase of 6.20 divisions for each degree of temperature is 517.0 divisions. Admitting that the observed value is correctly determined, and that the change for each degree is a real constant, there is but one way of reducing the relation 517.0 divisions to 490.5 divisions, viz., by reducing the temperature reading 4.26°. The thermometer reading at this point was therefore 4.26° too low. In a similar

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manner we may obtain the corrections of any mercurial thermometer at all points other than 0° and 100° on the centigrade scale and 32° and 212° on the Fahrenheit scale.

The particular bars of metal described have been chosen on account of the great number of comparisons between the relative lengths of the units traced upon them, which have been made since the year 1881. The results obtained previous to the present series of observations will be found recorded in the Proceedings of the American Academy of Arts and Sciences, Vol. XVIII, pages 287–398, and in the Proceedings of this Society for the year 1885. The agreement in the values of the various determinations of the relative coefficients of expansion of these metals are satisfactory in confirming the conclusions reached in this paper, and in establishing the fact that the relative coefficients have not undergone any change since 1881.

The bronze bar, which is designated R_2, has the same composition and cross section as the Imperial yard of Great Britain. It is composed of 16 parts of copper, 2½ parts of tin, and 1 part of zinc. Both the yard and the meter are traced upon platinum-iridium plugs securely inserted in the bar. Each unit of length is nearly standard at 62.0° Fahrenheit. The present investigation relates to the yard.

The yard and meter upon steel are traced upon polished steel plugs inserted in a bar one and a quarter inches in depth and three quarters of an inch wide. The material is the ordinary commercial steel as sold by the makers. This bar is designated R_3.

The glass bar designated G has the dimensions 41×1¼×1¼ inches. This bar is the property of the Standards Department of the British Board of Trade. It was made by Chance & Sons in 1870, upon a special order from the Department.

Another important limitation must be noted in this discussion. The results obtained all depend upon what are termed air contacts with the metals: that is, the bars of metal were supported upon the comparator at their neutral points in

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such a manner as to allow a free circulation of air at every point of their surfaces. The thermometers employed were placed upon the upper surfaces of the bars with a slight elevation of the ends farthest from the bulbs. After the bars had been adjusted for position side by side at the commencement of the series of observations, their positions remained undisturbed throughout the entire series. The stops of the comparator having been set at a distance apart approximately equal to the length of one yard, the length of the yard on each bar was compared with the distance between the stops. A single comparison usually occupied about five minutes, and during this time the constancy of the distance between the stops could be safely assumed, since this distance could only vary with the movement of the large mass of metal in the bed-plate of the comparator. By setting the microscope for coincidence with the initial line of each standard in succession, the comparisons for length were made without touching either of the bars. The nearest point of approach of the person of the observer to the bars was about 6 inches.

This explicit statement with regard to the manner in which the comparisons were made, and of the conditions under which the temperatures were obtained, is made in order that there may be no doubt concerning the basis upon which the conclusions reached in this paper rest.

It is stoutly affirmed in certain quarters that the method of comparison under air contacts is far less reliable than comparisons when both the bars compared and the thermometers employed to measure the temperature are submerged in a liquid. Even if the correctness of the method of air contact is admitted, it is insisted that the temperature should be measured by inserting the thermometers in wells sunk in the bars of metal and filled with some liquid, preferably mercury.

We have here three distinct problems, each one of the most difficult and intricate character.

First. When given masses of water, of alcohol, of mer-

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cury, or of glycerine are removed from a given temperature, and are submitted to contact with the air at a different and a practically constant temperature, in how long time will they reach the temperature of the surrounding air? If the time in which a thermometer will come to rest under similar conditions is less than this observed time, to what extent can a mercurial thermometer, when submerged in a liquid, indicate the real temperature of the liquid when both are undergoing unknown variations of temperature. This inquiry does *not* relate to the comparison of different thermometers by submergence in the same liquid. It *does* relate to the indication of the real temperature of the liquid mass. Any number of thermometers may give true *relative* indications of temperature when subjected to a common condition, and yet *all* may give erroneous indications of the real temperature of the liquid mass with which they are surrounded. This inquiry will also properly include a determination of the laws under which the different liquids reach an equilibrium of temperature with the surrounding air, especially in connection with the upward and downward flow of thermal currents, which are well known to exist in different parts of a given liquid mass.

Second. Under what circumstances and conditions will the mercurial thermometer indicate the temperature of the material with which it is placed in contact, and are the changes which take place in the indications given by thermometers under variations of temperature due to causes which relate to the thermometer itself, or to the changes of temperature in the material with which they are in contact. This inquiry will include a discussion of the time required for thermometers to pass from any constant temperature to another temperature also constant, under the three conditions, air contact, liquid contact, and surface contact with metals of different masses.

Third. When metals of different masses are subjected to contact with liquids, in how long time will they reach the temperature of the entire mass of the surrounding liquid, and

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is this time greater or less than the time required for equilibrium to take place when the metals are exposed to contact with the air under the same variations of temperature. Under this head will be included the determination of the time required for different masses of metal to reach the stationary point after the temperature of the surrounding air has become practically constant; and also the length of time which elapses after a change of temperature in the air has taken place, before the entire effect of this change is seen in a corresponding change in length.

It will be found advantageous to introduce this discussion with a few fundamental propositions relating to heat, and especially those which have a special bearing upon the special problem under consideration.

Fourier, in his classical treatise on the Analytical Theory of Heat, has given in simple language the fundamental conceptions relating to heat, which have formed the basis of all subsequent investigations. We quote such of these as are pertinent to our present inquiry:—

(a) "Heat, like gravity, penetrates every substance in the universe, its rays occupy all parts of space. The object of our work is to set the mathematical laws which this element obeys."

(b) Observation has shown conclusively that different bodies possess in different degrees:—

(1) The power to contain heat.

(2) The power to receive or to transmit heat across their surfaces.

(3) The power to conduct heat throught the interior of their masses.

(c) When heat is unequally distributed among the different parts of a solid mass, it tends to attain equilibrium, and pass slowly from the parts which are more heated to the parts which are less heated, and at the same time it is dissipated at the surface and lost in the surrounding medium. The tendency to uniform distribution, and the spontaneous

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emission which acts at the surface of bodies continually, changes their temperature at different points. The problem of the propagation of heat consists in determining what is the temperature at each point of a body at a given instant, supposing that the initial temperatures are known.

(d) If we place a solid homogeneous mass in the form of a sphere or a cube in a medium maintained at a constant temperature, and if it remains immersed for a long time, it will acquire at all of its points a temperature differing very little from that of the fluid. Suppose the mass to be withdrawn in order to transfer it to a cooler medium; heat will begin to be dissipated at its surface; the temperature at different points of the mass will not be sensibly the same, and if we suppose it divided into an infinite number of layers by surfaces parallel to its external surface, each of these layers will transmit at each instant a certain quantity of heat to the layer which surrounds it. If it be imagined that each molecule carries a separate thermometer which indicates its temperature at every instant, the state of the solid will from time to time be represented by the variable system of all these thermometric hights. It is required to express the successive states by analytical formula so that we may know at any given instant the temperature indicated by each thermometer, and compare the quantities of heat which flow during the same instant, between two adjacent layers or into the surrounding medium.

(e) Let us examine also the case in which a rectangular prism of sufficiently great thickness, and of infinite length, being submitted at its extremity to a constant temperature, while the air which surrounds it is maintained at a less temperature, has at last arrived at a fixed state which it is required to determine. All of the points of the extreme section at the base of the prism have by hypothesis a common and permanent temperature. It is not the same with a section distant from the source of heat; each of the points of this rectangular surface parallel to the base has acquired

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a fixed temperature, but this is not the same at different points of the same section, and must be less at points nearer to the surface exposed to the air. We see also that at each instant there flows across a given section a certain quantity of heat, which always remains the same since the state of the solid has become constant. The problem consists in determining the permanent temperature at any given point of the solid, and the whole quantity of heat which, in a definite time, flows across a cross section whose position is given.

(f) The solution of these problems has made us understand that the effects of the propagation of heat depend in the case of every solid substance on three elementary quantities, which are

(1) Its capacity for heat.
(2) Its own or interior conductivity.
(3) Its exterior conductivity.

(g) It has been observed, that if two bodies of the same volume, and of different nature, have equal temperatures, and if the same quantity of heat be added to them, the increments of temperature are not the same; the ratio of their increments is the ratio of their capacities for heat. . . . The proper or interior conductivity of a body expresses the facility with which heat is propagated in passing from one internal molecule to another. The external or surface conductivity of a solid body depends on the facility with which heat penetrates the surface, and passes from this body into a given medium, or passes from this medium into a solid. The last property is modified by the more or less polished state of the surface; it varies also according to the medium in which the body is immersed, but the interior conductivity can only change with the nature of the solid.

(h) Radiant heat, which escapes from the surface of all bodies, and traverses elastic media or even spaces void of air, and occurring with widely varied phenomena, follows special laws. To complete our theory it was necessary to examine the laws which radiant heat follows on leaving the surface of

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a body. . . . It results from the observations of many physicists, and from our own experiments, that the intensity of the different rays which escape in all directions from any point on the surface of a heated body, depends on the angle which their directions make with the surface at the same point. We have found that the intensity of a ray diminishes as the ray makes a smaller angle with the element of the surface, and that it is in proportion with the sine of the angle. . . . Up to this point omission has been made of the power which all surfaces have of reflecting part of the rays which are sent to them. If this property were disregarded we should have only a very incomplete idea of the equilibrium of radiant heat. . . . Thus, the reflection of heat does not disturb the equilibrium of temperature, and does not introduce, while the equilibrium exists, any change in the laws according to which the intensity of the rays which leave the same point decreases proportionally to the the size of angle of emission.

(i) Different *quantities* of heat are measured by determining how many times they contain a fixed quantity, which is taken as the unit. Suppose a mass of ice having a definite weight (1 kilogramme) to be at the temperature 0°, and to be converted into water at the same temperature 0 degrees by the addition of a certain quantity of heat. The quantity of heat thus added is taken as the unit of measure. . . . To raise a kilogramme of iron, for example, from the temperature 0° to the temperature 1°, a new quantity of heat must be added to that which is already contained in the mass. The number C which denotes this additional quantity of heat is the specific capacity of iron for heat; the number C has very different values for different substances.

(j) We take as the measure of external conductivity of a solid body a coefficient h which denotes the quantity of heat which would pass in a definite time (a minute) from the surface of this body into atmospheric air, supposing that the surface had a definite extent (a square meter), that the con-

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stant temperature of the body was 1°, and that of the air 0°, and that the heated surface was exposed to a current of air of a given invariable velocity. This value of h is determined by observation. *The quantity of heat expressed by the coefficient is composed of two distinct parts, which cannot be measured except by very exact experiments, one is the heat communicated by way of contact to the surrounding air, the other much less than the first, is the radiant heat emitted.*

(k) All bodies have the property of emitting heat through their surface; the hotter they are the more they emit; the intensity of the emitted rays changes considerably with the state of the surface. . . . Every surface which receives heat from surrounding bodies reflects part and admits the rest; the heat which is not reflected but introduced through the surface, accumulates within the solid; and so long as it exceeds the quantity dissipated by irradiation the temperature rises. . . . The rays which tend to go out of heated bodies are arrested at the surface by a force which reflects part of them into the interior of the mass. The cause which hinders the incident rays from traversing the surface and which divides these rays into two parts, of which one is reflected and the other admitted, acts in the same manner as the rays which are diverted from the interior of the body towards external space. . . . The incident rays introduced into the mass and the rays emitted from the surface are equally diminished in quantity.

(l) Liquid substances possess also the property of transmitting heat from molecule to molecule, and the numerical value of their conductivity varies according to the nature of the substances, but this effect is observed with difficulty in liquids, since their molecules change places under changes of temperature. The propagation of heat in these depends on this continuous displacement in all cases where the molecules of the mass are most exposed to the action of the source of heat. If, on the contrary, the source of heat be applied to that part of the mass which is highest, as was the

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case in several of our experiments, the transfer of heat, which is very slow, does not produce any displacement, at least when the increase of temperature does not diminish the volume, as, indeed, noticed in singular cases bordering on changes of state."

To these quotations from Fourier may properly be added the statement of Maxwell, that "the phrases radiation of heat and radiant heat are not quite scientifically correct and must be used with caution. Heat is certainly communicated from one body to another by a process which we call radiation, which takes place in the region between the two bodies. We have no right, however, to speak of this process of radiation as heat. We have defined heat as it exists in all bodies, and we have seen that all heat is of the same kind. But the radiation between bodies differs from heat (1st), in not making the body hot through which it passes; (2d), in being of many different kinds. Hence, we shall generally speak of radiation, and when we speak of radiant heat we do not mean simply the existence of a new kind of heat."

We close this enunciation of fundamental principles relating to heat with Ganot's statement of *Prevost's Theory of Exchanges:*

"All bodies, whatever their temperature, constantly radiate heat in all directions. If we imagine two bodies of different temperatures placed near each other, the one at a higher temperature will experience loss of heat, its temperature will sink, because the rays it emits are of greater intensity than those it receives; the colder body, on the contrary, will rise in temperature, because it receives rays of greater intensity than those which it emits. Ultimately the temperature of both bodies becomes the same, but the heat is exchanged between them, *only each receives as much as it emits, and the temperature remains constant.*

Fourier shows that if we suppose a solid body formed of a homogeneous substance to be included between two parallel and infinite planes, A and B, separated by the dis-

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tance e, and if the lower surface A has a constant temperature a, while its upper surface B is maintained at a constant temperature b, less than a, and if both a and b retain their values for an infinite time, then for any temperature v at the distance z from the lower plane, the decrease in temperature between A and the point z will be expressed by the equation

$$v-a=\frac{z}{e}(b-a)$$

and the quantity of flow F of heat which crosses a definite part of a section parallel to the base A during a given time will be expressed by the equation

$$F=K\left(\frac{a-b}{e}\right)$$

in which K is the measure of the specific conductivity of the substance from the base A to the section z. He also shows that if the upper surface B permits the escape into the air of a certain quantity of heat just equal to that which crosses any cross section of the solid in flowing from A to B we shall have:

$$F=h\ (b-\beta)$$

where h is the surface conductivity of the metal. This quantity is just equal to the quantity given by the expression

$$K\left(\frac{a-b}{e}\right)$$

or

$$K\left(\frac{a-b}{e}\right)=h\ (b-\beta)$$

from which β, the temperature of the exterior medium at the surface of the solid, can be determined when K and h are known.

*Fourier then extends the investigation to the measure of the movement of heat at a given point of a solid, bounded by six rectangular planes, pairs of which are parallel, and gives the following general equation to determine the thermal condition of any point whose position is determined by the co-ordinates x, y and z:

* See Translation of Freeman, p. 79.

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$$v = A + ax + by + c\,z$$

in which A represents the surface temperature, and a, b and c represent certain coefficients, which are functions of x, y and z, respectively. He finds that the flow of heat along the different axes is:

$$-k\frac{dv}{dx}\,\omega\ dt \text{ along the axis of } x$$

$$-k\frac{dv}{dy}\,\omega\ dt \text{ along the axis of } y$$

$$-k\frac{dv}{dz}\,\omega\ dt \text{ along the axis of } z$$

in which ω represents the area of a circle of infinitely small thickness and having an infinitely smaller diameter, through which the heat passes from the under side to the upper side during the instant dt; and k represents the internal conductivity which holds for this particular section ω.

We thus have in actual practice three definitions of thermal conductivity, viz:

(1) h=the coefficient of surface conductivity.*

(2) k=the coefficient of the internal conductivity for the elevation corresponding to the height z above the lower plane.

(3) K=the mean coefficient of conductivity for the limit between the base A and the height z.

Two facts are of special importance in this discussion — first, that the flow of heat, or the temperature effect shown in the increase of the length of the bar, is a function of the time of exposure; and, second, that the temperature v can only be computed when we know the surface temperature A and the constants h and k. The complete separation of h from k is practically impossible, and the determination of A presents great difficulties. This difficulty is clearly stated by Sir William Thomson, who, in speaking of the flow of heat from a metal into the surrounding medium, says: "Hence,

* The term conductivity is here used in the sense of flow of heat, and does not necessarily involve the idea of contact with a hotter or colder metal.

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although the water or the air at the very interface of its contact with the metal, is essentially at the same temperature as the metal, there must be a great difference of temperature in very thin layers of the fluid close to the interface, where there is a large flux of heat through the metal, and the temperature of the fluid as measured by any practical thermometer, or inferred from a knowledge of the average temperature of the whole fluid, or the temperature of entering and leaving currents of fluids, may differ scores of degrees from the true temperature of the solid at the very interface."

It is generally assumed that the thermal conductivity of iron is 80 times and that of copper, 500 times that of water, and that the thermal conductivity of iron is 3500 times, and of copper 20000 times that of air. It may be doubted whether water is the proper unit of comparison, since its thermal conductivity depends on molecular action which is radically different from that which takes place in metals. In any event, since the various methods of measuring the thermal conductivity depend ultimately, as stated by Tait, "upon observation of temperature of the conducting body at different parts of its mass," and since the element, time of exposure, is exceedingly difficult to manage, even if it is not a real variable, it may well be doubted whether we have at present sufficiently accurate data to secure a practical realization of the analytical solution of the problem given by Fourier. This uncertainty is well illustrated in the divergence of the values of the conductivity of copper determined by Péclet, Clément and Augström. Admitting that the value 1.1 found by the latter physicist to be the true one, the value found by Clément is nearly 200 times, and that of Péclet five times too small.

It is generally assumed, also, that the value of the conductivity varies with the temperature, but we can hardly define the second term of the law in question, as long as so much doubt remains concerning the first term.

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The value of the thermal conductivity of different metals, determined by Péclet, Despretz, Tait, Weidemann, and Franz, were founded on the uniform conduction of heat across slabs of metal and along their bases by measuring the absolute quantities of heat that were *conducted out of the substances.* On the other hand the measures of the thermal conductivity, made by Thomson and Forbes, were founded on the laws of the diffusion of heat through bars of iron and of copper placed under ground. Hence, since the conditions of outward flow from the interior are different from the inward flow from the air into the interior, it is evident that the constant of surface conductivity h, must have a different signification and different values, according to the direction of the flow. Calling the outward flow h, we may call the inward flow h^1. Thomson calls the constant h^1 the thermal diffusivity. It expresses the ratio between the thermal capacity and the conductivity of the metal, since the thermal effects are inversely as the thermal capacity of the body.

The same distinction holds in the case of the outward surface flow h, which may be properly called the coefficient of thermal emissivity, or simply the emissivity, according to Thomson.

It is evident that the method of Thomson is more nearly applicable to our present problem, viz., that of the flow of heat in bars of metal having widely different capacities for heat and subjected to varying changes of temperature under air contact. Mathematically considered our problem is this:

Given two or more bars of different specific capacities for heat, supported in air, and having a constant temperature v; what will be their relative thermal conditions after they have been submitted to another constant temperature v^1, for a time T.

The quantity of heat which flows per unit of time across any transverse section of the bar will be the product of its surface conductivity h, of the area of the cross section, and of the gradient of the temperature of that cross section, which may be taken as equal to the difference of the tem-

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peratures of the two sides of a layer of unit thickness. If we represent the distance of the first layer from the surface by x, the temperature will be $\frac{dv}{dx}$ and the inward flow will be $\frac{h^1 dv}{dx}$. For any distance $x+dx$ the flow becomes

$$*\frac{h^1 dv}{dx}+\frac{d}{dx}\left(\frac{h^1 dv}{dx}\right)dx.$$

The rate at which the temperature will change will be expressed by $\frac{dv}{dt}$. If we represent by c the thermal capacity of the surface, the flow will be expressed by $\frac{cdv}{dt}$.

Hence $$\frac{cdv}{dt}=\frac{d}{dx}\left(\frac{h^1 dv}{dx}\right)$$

For equilibrium we have $\frac{dv}{dt}=0$ and $\frac{d}{dx}\left(\frac{h^1 dv}{dx}\right)=0$

and hence we have a linear equation of the form—

$$v=A+Bx$$

in which A represents the surface temperature, and B represents the ratio at which the temperature changes per unit of distance in a direction perpendicular to the surface of the bar.

We here meet with precisely the same difficulty as before, viz., our inability to measure the surface temperature at the very interface and to separate the thermal conductivity B from the constant A.

It is obvious that if these theoretical considerations hold true, we do not obtain the real quantity A, which we desire, by immersing the thermometers in wells sunk in the bars and filled with mercury. This statement is true independently of any question which may be raised with reference to the ability of mercury to take the temperature of the interior mass of the metal.

That the outward and inward flow A, measured by contact of the mercury bulb of the thermometer with the surface

* See Tait on Heat, p. 219.

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of the bar, represents the temperature-effect in changing the length of the bar is shown pretty conclusively by the following experiment. A bar of metal is placed upon the comparator, supported in air at its neutral points and at a constant temperature, *e.g.*, of 32° Fahr. The presence of the observer in the vicinity of the bar will have no apparent effect in changing the length of the bar for a period of fifteen or twenty minutes. But if the observer presses his hand upon the upper surface of the bar for 30 seconds a certain amount of heat will be conducted into the bar *which will produce nearly the entire change in length which will take place, almost at the very instant of contact*, showing that equilibrium between the internal heat imparted and the irradiation at points on the surface different from the point of contact was established nearly instantaneously.

If the hand is removed, the shortening due to cooling, will become evident within fifteen or twenty minutes in the case of the bronze bar R_2, and in a time a little less in the case of the steel bar R_3. The heat effect will be entirely dissipated in the course of three or four hours. If the experiment is tried upon a thin strip of metal with large surface area, e.g., with a thickness of one hundredth of an inch, the return to the original condition will occur within a very short time.

If, continuing our experiment, the hand is again pressed upon the bar, after an interval of five minutes, for a second interval of 30 seconds, the bar will become instantly charged with a second increment of heat, which will result in another increase of length, nearly, but not quite, equal to the increment due to the first charge. As many as ten charges have been made at intervals of five minutes, after which it was found that the total change in length was very little different from ten times the amount due to the first charge. It was also quite unexpectedly found that the time required for the dissipation of the ten charges did not differ much from the time required for the dissipation of the first charge.

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We are now prepared to return to the inquiries made upon page 17. We have shown that we do not need to determine by what particular process and under what laws internal conduction has taken place in our bars, since the temperature-effect of any increase in temperature in increasing the length of the bar has, to a large extent, its equivalent in the amount of heat which passes the surfaces of the bars by an inward flow, and since an equilibrium between the temperature of the entire mass of the bars with that of the surrounding air has become established. Moreover, under the theory of exchanges, when this equilibrium occurs, *we may neglect the effect of radiant heat altogether.*

We may therefore, employ the mercurial thermometer, placed horizontally upon the surface of the bar, to measure the amount of heat which produces the linear expansion observed. Even if this conclusion should be denied, and it should be insisted that liquid contacts are to be preferred, it may still be shown that it is well nigh impossible to determine with sufficient precision the time when the entire mass of a surrounding liquid is in equilibrium with the entire mass of the metal submerged. This remark applies both to the bars to be compared and to the thermometers employed in making the comparisons when the quantity required is the *real temperature of the liquid.*

This conclusion may be fairly drawn from quotation (1) from Fourier. In actual practice it will be found that while the first effect of liquid contact with a bar of metal is to cause it to approach the normal relation which will exist when equilibrium is established with great rapidity, this relation is reached *permanently only when the entire mass of the liquid and that of the metal have reached the temperature of the surrounding air.* It will be found that the time required for this condition to be fulfilled is considerably greater than that required for the metal to reach the same condition under air contact. When two bars of different thermal capacities are submerged in water at a temperature

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lower than that of the surrounding air, the error in the determination of their relative lengths will, in spite of every precaution, tend uniformly in one direction. When, under similar conditions, the temperature of the liquid is higher than that of the surrounding air, the actual error of the derived relation will tend uniformly in the opposite direction.

The method of investigation pursued in our first inquiry has been to remove given masses of water and of mercury, placed in vessels having different diameters and composed of different materials, from a nearly constant low temperature to a comparing room in which a nearly constant air temperature could be maintained. The process was then reversed, the liquids passing from a nearly constant air temperature to complete immersion in melting ice. These experiments are still in progress. The details for two exposures are given in exhibit (A). The discussion of the results is properly omitted until the completion of the entire series of observations. This series will include exposures in vessels of glass, tin, ebonite, etc., in vessels both open and closed at the top and covered with blackened caps.

Our second inquiry may be introduced by the relation of an experience of the writer in repeated comparisons of the relative lengths of the steel and bronze standards, made during the years 1884 and 1885. In January of 1884, the writer constructed upon Jessup's steel, three combined yard and meter units of length of the form described in Vol. XVIII., page 290, of the Proceeding of the American Academy of Arts and Sciences. Yard and meter B was made for Professor Brackett, of Princeton College. Yard and meter A_4 was constructed for Professor Rowland, of Johns Hopkins University. Yard and meter R_3 was retained for my personal use. In the discussion of these standards it was found that the equation between the Brackett standard B, and my bronze standard R_2, determined in 1883, differed by a sensible amount from the relation found in 1884. It was also found that the relation between R_3 and R_2, deter-

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mined in the early part of 1883 differed materially from that found in the latter part of the same year. In order to obtain additional data in regard to the constancy of the relation between these three standards, Professor Rowland kindly returned to me standard A_4 in order that I might be able to verify the comparisons with R_2, made in 1884. The results of the new series are given in exhibit (B). The equations given relate to the relations between standards R_3 and A_4 and the *Metre des Archives* A_0 for the meter, and with the Imperial Yard Y for the yard. With regard to the results of this series I repeat here the conclusions stated in my paper on *The determination of the relative lengths of eight Rowland gratings, printed in the Proceedings of the Society for 1885, page 161.*

"It may be said that the disagreement in the results given in exhibit (B) is not much greater than the probable error of observation, except in the case of the equation $R_3 - 1.2\mu = A_0$. Here the agreement between the separate comparisons forbids the assumption of accidental errors as the cause of the variation. Besides, the disagreement between the different values of the relation R_2—B; omitted here on account of the incompleteness of the first set of observations, is even greater than for $R_2 - R_3$.

In all of the comparisons made since 1884, especial pains have been taken to eliminate the error arising from the failure of the thermometer when placed upon the upper surface of the standards to indicate the real temperature of the metal. Even when a thermometer reading seems to be stationary, it has a *drift* up or down. The direction of this drift is always noted, and the aim has been to combine the observations in such a manner as to balance the errors due to this cause. Very gradually, however, the conviction has been forced upon the writer, that when the temperature of a considerable mass of metal is obtained from the reading of a thermometer placed upon its surface, errors of long period are always introduced which escape detection in a short

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series of observations, but which manifest themselves in a long series in passing from winter to summer and *vice versa*. This view must at present be taken as a working hypothesis; but if it should prove to be correct, it will have an important bearing upon the work already done in metrology.

In order to test this theory it is absolutely necessary that the comparisons shall be made in a room in which a steady temperature can be maintained for a long period of time, and in which the changes which occur are very slow. Otherwise, it would be impossible to separate the errors of short period from those of long period. Through an appropriation from the Rumford Committee of the American Academy of Arts and Sciences, a comparing room was built beneath the rotunda of the Observatory which nearly fulfulls the required conditions. The full account of the work undertaken by Mr. A. L. McRae and by the writer, under this appropriation, will appear in a report to the Academy, but by permission the results of the comparison between R_2 and R_3 are given here.

Arranging the comparisons in groups of ten each, we have the following result:

From	May 25	to	May 27,	$R_2 - R_3 = +5.32\mu$
"	May 28	to	May 31,	$= +4.16$
"	May 31	to	June 2,	$= +4.81$
"	June 3	to	June 4,	$= +4.00$
"	June 5	to	June 16,	$= +4.05$
"	June 16	to	June 27,	$= +2.23$
"	June 28	to	July 8,	$= +2.34$

If these observations can be trusted, we have here a decided diminution in the value of the relation $R_2 R_3$ between May and July, notwithstanding the most careful efforts to eliminate all sources of error. The temperature during this interval gradually increased from 9° to 15° C. It is also to be distinctly noted that these variations *inter se* are much greater than we should naturally expect under the variations of temperature which ordinarily occur.

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It is evident that we have here under another form, a problem which has, so far, baffled every attempt at solution. The difficulty encountered is well illustrated in the diversity of the results which have been obtained for the relation between the yard designated "Bronze 11," which is the virtual standard of the United States, although it has received no legal sanction, and the Imperial Yard of Great Britain. When this yard was presented to the United States by the British Board of Trade, in 1856, it was declared to be standard at 61°.79. It was, therefore, fourteen hundred thousandths of an inch *too long*. Within a few years two series of direct comparisons between these two standards have been made as the result of which it appears that "Bronze 11" is eighty-eight millionths of an inch *shorter* than the Imperial Yard. The discrepancy has been regarded as a well-authenticated instance of an actual change in length; but, Pierce, in 1882, found "Bronze 11" to be only eleven millionths of an inch shorter than the Imperial Yard. The evidence of change of length due to age of the metal is, therefore, not conclusive, unless it is admitted that changes in two directions have taken place.

It became evident, early in this investigation, that while a given series of comparisons might give results which would agree *inter se* within very narrow limits, another series made at a different time of the year, would give a final result, which, in many cases, differed from the first result by an amount far greater than that due to the accidental errors of observation. It soon became apparent that these observed variations were confined to bars of metal having widely different coefficients of expansion, and having widely different masses.

During the latter part of the year 1885, the necessity for an organized search for the cause of these abnormal variations became so manifest that it was deemed useless to continue further comparisons of the standards of length under the system employed up to this time. There were many evi-

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dences that the abnormal variations in question were in some way connected with the indications of the temperature by the thermometer employed. It was therefore resolved to make a special study of the individual thermometers, simply as instruments designed to record the temperature and without any relation to their use in connection with comparisons for length.

The investigation which follows was made possible by the construction of a comparing room beneath the rotunda of the Observatory of Harvard College, from the avails of an appropriation made for this purpose by the Rumford Committee of the American Academy of Arts and Sciences. The details of the work will appear in a report to the Academy, but by permission, the general results of the investigation are given here.

The following working hypothesis was assumed as a basis for this investigation. The height of a column of mercury in the tube of a thermometer depends on the quick action of the temperature in expanding the mercury in the bulb, and the slow action upon the column of mercury, which takes place through changes in the temperature of the stem, and especially of the metal framework upon which the thermometer may be supported. The resultant of these forces, acting sometimes in unison with each other, but more often in opposition to each other, produces slight oscillations in the height of the column, which vary with the relative masses of the bulb, including the mercury and the stem. A thermometer whose bulb is composed of a very thin shell may be very sensitive, but this good quality may be neutralized by the liability to changes due to the heat or cold stored up in a thick stem. Pulsations may also occur on account of the abrupt variations in the diameter of the bore. The action here indicated is to be distinguished from the instantaneous fall of the mercury column due to a very sudden increase of temperature resulting in an increase of the diameter of the bulb before the change has time to act upon the mercury

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within the bulb. In any event, any particular thermometer selected is liable to contain idiosyncrasies which must be carefully studied before reliance can be placed upon its indications.

In order that the conclusions reached might, if possible, be applicable to thermometers as a class rather than to individual thermometers, the following thermometers were placed in the series to be compared.

(1) A Yale Centigrade standard, by Tonnelot, designated Y_{62}.

(2) A Normal Centigrade standard, by Warmbrum, Zuilitz & Co., of Berlin. This thermometer, designated G_1, is the property of Wellesley College.

(3) A Normal Centigrade standard, by Weigand, of Wurzburg. It is designated G_2 and belongs to Professor Goodale, of Harvard College.

(4) A Normal Fahrenheit, by Baudin, designated B and standardized at Breteuil.

(5) A spirit thermometer, by Huddleston, of Boston, designated H.

(6) A spiral bulb thermometer, also by Huddleston, designated S.

(7) A Fahrenheit standard, by Green, designated G_3.

(8) A Fahrenheit standard, by Cassella, of London, designated C.

(9) Three spiral metal thermometers on the Briguet principle, made by the Standard Thermometer Co., of Peabody, Mass.

Since it is at least a plausible supposition that there may be errors peculiar to all mercurial thermometers as a class, a form of metal thermometer was employed from which fairly good results were obtained. Strips of steel and silver were stretched from a common point of support side by side, the tension being produced by spiral springs. The strips were one inch in width and their thickness was not far from one hundredth of an inch. At the distance of one meter from the

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point of support a common line was traced across the surface of each strip. It was therefore possible to measure the relative expansions of the steel and the silver strips by measuring with the micrometer of the microscope, the amount of separation between the two lines.

This form of thermometer was found to be far more sensitive to sudden changes of temperature than the mercurial thermometers, but the certainty of the return to the zero point after wide excursions, cannot be considered as definitely established. The degree of constancy will be seen from the record given in exhibit (C).

Inasmuch as the strip thermometer was found to be much more sensitive to changes of temperature than the mercurial thermometers, the agreement of the separate values of Δ is quite as close as ought to be expected. It will be seen that the values of Δ are larger than the mean value when the temperature is rising, and smaller than the mean value when it is falling. From ten observations with a rising temperature the value of Δ is 64.7 divisions of the micrometer. From ten observations with a falling temperature $\Delta = 57.2$ divisions. The mean value is 59.9 divisions, which agrees closely with the mean value derived from the observations made at a stationary temperature, viz., 57.7 divisions. In this series therefore the zero seems to be constant. It should be said, however, that in several preceding series the departure from the established zero occasionally amounted to nearly 1°, especially when the tension of the strips was evidently too great. It will be seen from the solution of the equations of condition that the final residuals indicate a liability of error which does not much exceed two tenths of a degree.

The entire number of comparisons of thermometers made during the year 1886 is about 22,000. Since this work is to a large extent in the nature of a diligent search after unknown quantities, the observations have been divided into a series of groups, in each of which some particular inquiry has

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been the leading feature. The conclusions reached in each series will be given in the order of the observations upon which they are based. It will be observed that these conclusions are, in several cases, modified by subsequent observations and it is quite likely that further modifications will be necessary.

Our problem may be stated as follows:

Can the variations of standards of length from their normal relations, observed at different seasons of the year, be attributed to systematic errors in the indications of the thermometers with which the temperature is determined?

The normal relations between the different thermometers, compared under normal conditions, were first obtained in order that they might serve as a basis of comparison for the abnormal relations. The results obtained depend upon about 8000 comparisons. An examination of these results shows that we must always expect errors of reading varying from 0.1° to 0.3° C, but these errors are largely due, as shown by the observations of 1887, to the lack of attention to the necessity of making the comparisons at the time when there is an equilibrium between the temperature of the thermometers and that of the surrounding air.

There can be no better test of the character of the indications given by a thermometer, than the determination of the time required to pass from a normal reading at one constant temperature to a normal reading at a different constant temperature. The determination of this time, for the different thermometers, constitutes our first inquiry. The method of observation was as follows: All the thermometers exposed were allowed to come to rest under one of the conditions selected, which we will call condition A. One of the standard thermometers, generally Y_{62} remained permanently at the temperature of the condition to which the thermometers were to be removed, which is designated condition B. After the removal of all the thermometers, except one from A to B, the comparisons with the thermometer at B were made at

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intervals of about five minutes. After a complete equilibrium between all the thermometers had been established for condition B, they were restored to condition A, and similar comparisons were made with the standard which remained under condition A.

SERIES I — Condition A. Thermometers suspended from wires in open air at — 10° Fahr.

Condition B. Thermometers rest horizontally upon a shelf in the Prime Vertical Room of Harvard College Observatory, in which a very constant temperature of about 5° Fahr. was maintained for several days.

Conclusions. Normal relations reached in times varying between nineteen minutes and four hours. Movement by pulsations at several points very marked. The column of mercury often stationary for a long time, followed by sudden upward or downward movements. The mercurial thermometers all pass the point of their normal relation in times, somewhat proportioned to the mass of the mercury in the bulb, and the thickness of the stem. These times vary between 19^m and 25^m. The time required to overcome this excess of reading varies from 1^h to 4^h. All the thermometers have nearly the same degree of sensitiveness, except the Cassella standard. The metal thermometers reach the normal relation a little in advance of the mercurial. The metal and the mercurial thermometers agree closely at the low temperatures.

SERIES II — Condition A, same as condition B in Series I. Temperature — 10° C.

Condition B. Thermometers rest horizontally upon a shelf in the clock-room of the Observatory, in which the variations in temperature during twenty-four hours does not much exceed 0.5°.

Conclusions. All the thermometers, both mercurial and metal, pass the normal point, and the time occupied till the return varies between two hours and forty-eight hours. In two or three instances the normal point was passed a second

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time in the opposite direction. Slow oscillations occur at different points and with great irregularity in regard to time. Y_{62} always reaches the normal point in advance of the other thermometers; the spirit thermometer, which has a very large cylindrical bulb, always falling behind, but the difference does not much exceed four or five minutes. The average time required to recover the distance passed beyond the normal point is nearly the same in all cases, except with the Cassella thermometer. *First noticed that the amount of running by, and the length of time required for the return is, to a certain extent, proportioned to the range of temperature passed over, but is proportionally greater for slow than for rapid changes.* A large number of instances occur, in which all the thermometers agree in giving erroneous results after passing the normal point varying in amount from 0.2° to 0.4° C. The persistence with which these abnormal variations occur shows that they are not due to accidental errors of observation.

SERIES III — Condition A. Thermometers immersed in melting ice.

Condition B. Thermometers rest horizontally on metal bars, which rest upon the comparator in the new comparing room beneath the rotunda of the Observatory. Mean temperature about 15° C.

Conclusions. Time of passing from 0° to 15° varies from 17 to 25 minutes. Marked evidence of the truth of the inference, drawn from Series II, that the abnormal variations which occur after the thermometers pass the normal point are much greater for very slow variations than for rapid changes, and also that the time of recovery after passing the normal point is much greater for a change of 15° C. than e. g. for a change of 30° C.

SERIES IV — Condition A. Thermometers immersed in melting ice.

Condition B. Same as in Series III, but the constant temperature of the comparing room was about 38° C.

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Conclusions. Very slight evidence of passing the normal point. The time required to pass from 0° to 38° nearly the same for all the thermometers except Cassella and the Huddleston spiral thermometer, viz., from 17 to 25 minutes. Metal thermometers less sensitive than the mercurial for a few minutes after removal, but they reach the normal point in about the same length of time. This series offers pretty conclusive evidence that the time required to pass from one temperature to another constant temperature is nearly the same whatever the range of temperature passed through, but that the recovery after passing the normal point varies inversely as the range and directly as the relative masses of the bulb and of the stem.

SERIES V. The thermometers in this series were supported in air at their neutral points upon pieces of tin set edgewise.

Conclusion. The most important result from this series is the apparent demonstration of the fact that under a gradually increasing temperature all the mercurial thermometers read too low by amounts varying between 0.1° and 0.4° C. and too high by about the same amounts during a slowly decreasing temperature. In other words they fail to take the temperature of the surrounding air while these slow changes are occurring.

SERIES VI — In this series part of the thermometers rested horizontally upon bars of metal and part were supported in air.

Conclusions. Under slow changes of temperature there is a very decided evidence that the temperature of the bars hold the thermometers which rest upon them in check, and that they only reach the normal relation after several hours. The strip thermometers give indications far in advance of the mercurial thermometers, even indicating a falling temperature while the mercurial thermometers indicate a rising temperature, and for a period of 20 minutes after the change in temperature takes place.

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SERIES VII. This series was undertaken to determine how far, under a rapid rising temperature, the mass of metal upon which the thermometers rest may hold them in check as shown by comparison with a thermometer supported in air at about the same height. On July 4, at 7^h 40^m A. M. it was found that thermometers G_1 and G_2 read 0°.93 and 0°.73 respectively higher than the corrected value of Y_{62}. These values represent the real corrections required at 15° C. G_1 was supported in air, while G_2 and Y_{62} rested upon a large mass of metal, weighing about 100 lbs. At 8^h 11^m the gas under the boiler outside of the comparing room was lighted. The hot water from this boiler had a steady flow through iron pipes in the comparing room, returning to the upper part of the tank. The effect of the mass of iron in holding thermometers Y_{62} and G_2 in check will be seen from exhibit (D).

It appears from this experiment, that, under a rapid change of temperature in the comparing room, thermometer G_1 in air was nearly 8° in advance of Y_{62}, while G_2 was only a little over 1° in advance after an interval of 2 hours. This excess continued with a diminishing ratio for nearly an hour after the heat had been withdrawn from the boiler. At twelve o'clock, four hours after the commencement of the series, G_1 was 3°.5 ahead of Y_{62} and 0°.6 ahead of G_2. At no time, therefore, during an interval of 4 hours, did either of the thermometers, placed upon the iron, indicate the temperature of the surrounding air.

While the observations made during the year 1886 were of great service in indicating the conditions under which temperature should be determined, it cannot be said that they offered any decisive evidence in regard to the real solution of the problem in hand.

It was, therefore, determined to make a new series of comparisons under entirely different conditions. The series for 1887 was made after my removal from Cambridge to Waterville, Me. The plan of observations involved the comparison

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of 30 Fahrenheit thermometers of the Signal Service pattern made by Green, of New York, with centigrade standard Y_{62}, and a Green centigrade standard.

These thermometers were divided into six groups of five each. In the in-door series they were suspended at intervals of about two inches between the groups, from a long bar which was itself suspended near the middle of a large room, at the distance of about five feet from the floor. In the out-door series the thermometers were suspended upon a revolving frame about ten feet from the ground. The standard cylindrical bulb thermometers were suspended horizontally, first a little below the spherical bulbs of the Fahrenheit thermometers, and afterwards at different distances above the bulbs, The readings were at first made with an objective of two inches aperture from a distance of about twelve feet, and afterwards with a telescope of four inches aperture and from a distance of forty feet. Thus far, about 17000 comparisons have been made, and it is proposed to continue the observations for one year. Already comparisons have been made between the extreme temperatures of $-30°$ and $+95°$ Fahr.

In this series of observations a radical departure from former methods of comparisons was made.

Reasoning from the laws of the flow of heat between the thermometers and the surrounding air enunciated by Fourier, it seemed to me that most, if not all the errors due to our imperfect knowledge of the laws of the conduction of heat, and especially to our inability to distinguish between the flow of heat from the interior of a mass of metal outward from its surfaces into the air, and the radiant heat which escapes from the surface during slow changes of temperature, would be at least reduced to a minimum, if all the comparrisons were made at the critical point of equilibrium, or, as I may call it, the critical point of quiescence when the entire mass of the thermometer is at the same temperature as that of the surrounding air.

Under the third inquiry it appears, from the observations

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given under exhibit (E), that when steel and bronze bars are compared while the air is undergoing a slowly rising temperature, the steel standard, which has a less cross-section than the bronze, becomes relatively *longer* than the bronze, while for a slowly decreasing temperature it becomes relatively *shorter* than the bronze standard. The difference may amount to as much as five or six mikrons. Reasoning in the same way, as in regard to the thermometers, it seemed reasonable that at the point of perfect equilibrium between the temperature of the air, the entire mass of the metals and of the thermometers placed upon their upper surface, we ought to obtain nearly an ideal condition in which the effects of conduction, radiation and irradiation need not be considered. At any rate, it seemed possible to eliminate the errors in question by combining observations made, as nearly as possible, at equal distances on each side of the critical point of no variation. By determining this critical point for air and for various liquids, we ought, in every case, to obtain the same result as would be obtained if we could employ an ideal fluid and a thermometer of ideal properties under conditions in which actual changes of temperature would be recorded instantaneously. Now two of these critical points of equilibrium occur in nearly every day in the year: one in the early morning, and the other late in the afternoon. It was not found possible to make use of the one which occurs in the afternoon in our particular problem; but in the early morning this point is well defined. At Waterville, under the conditions which there prevailed, at least during the years 1886–7, the time of equilibrium occurs about half an hour after sunrise, on clear days, and somewhat later on cloudy days. The time for thermometers is considerably earlier than for the standard bars.

Nearly all of our difficulties therefore ought to vanish by the adoption of the simple plan of making all comparisons, both of thermometers, and of standards of length symmetrically with respect to the critical point of quiescence.

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After an experience of several months I feel safe in saying that they *do* vanish.

That part of the discussion which relates to the comparisons of the thermometers themselves *inter se* is now no longer germain to our present inquiry. It will be reserved therefore for a subsequent communication. It will be sufficient to state here four of the conclusions which seem to be established by the observations which have been made up to the present time.

(a) The time required for thermometers to pass from one constant temperature to another temperature also constant, varies to a certain extent with the form of construction of the thermometers, and is nearly the same whatever the range of temperature. For thermometers Y_{63}, B and G_1 this time is not far from 17 minutes. The longest time required is about 26 minutes. The time required for the spiral metal thermometers is about 20 minutes, while for the metal strip thermometer it is decidedly less than for the mercurial thermometers.

(b) The tendency to pass the normal relation during a change from one constant temperature to another constant temperature, is due to a far greater extent than was at first suspected to the conduction effect of contact of the thermometers with metals of large mass, upon which the thermometers rest. The supposition that the slow action of heat through the stem and especially through the metal framework of the thermometers would account for the slow oscillations of the mercury column observed, still holds true, but to a less extent than at first supposed, the larger share of this effect being due to the control which the temperature of the metal bars upon which the thermometers are placed holds over the temperature registered by the thermometers.

The truth of this statement is clearly shown by noting the time in which a series of thermometers will pass from 0° C. to the constant surface temperature of a large mass of metal for the angular positions of the stem with respect to the metal at 0°, 20°, 40°, 60° and 90°.

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(c) Admitting that the real temperature at any instant, is registered by the cylindrical bulb thermometers Y_{62} and G, the first indications given by the spherical bulb thermometers with a supporting metal frame-work are entirely untrustworthy. Careful observations show that when a change from the stationary point of equilibrium takes place, as indicated by Y_{62} and G, the readings of 30 Fahrenheit thermometers pass from a variation, not exceeding 0.1°, to a variation of about 0.8°. The extreme normal mean deviation of these 30 thermometers at 30°, 50°, 60° and 80° from Y_{62} and G, is certainly less than 0.1°, and yet within 30 seconds after a change has been noted in Y_{62} or G, they often exhibit deviations of from two or three tenths of a degree between different groups, and of from six to eight tenths of a degree between the individual thermometers. In case the temperature again becomes constant and remains so for about fifteen minutes the original agreement is restored.

The same result has been obtained by having an assistant walk back and forth behind the thermometers in equal intervals of time, while the effect of the heat thus communicated was observed with the aid of the reading telescope.

(d) The indications given by thermometers which are themselves undergoing changes of temperature, are uncertain within limits varying between 0.1° and 0.4° C. The necessity of making all comparisons at the stationary point of no variation of temperature is quite as great as for the comparison of standards of length under the same conditions. The length of time, however, during which the temperature must remain constant before the comparisons can be safely made, appears to be about 20 minutes for air contacts. The time required for liquid contacts depends on whether the object is to compare thermometers *inter se* under identical conditions, or, which is quite another thing, to determine the real temperature of the liquids in which the immersion takes place. The experiments, which are now in progress, point unmistakably to the conclusion that equal quantities of water

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and mercury pass from a given constant temperature to another temperature, also constant, in nearly the same time, but with widely different rates of flow. If subsequent observations confirm this conclusion, it is evident that we may substitute for the specific heat of a substance which Thompson defines as the thermal capacity of a stated quantity of it, the specific rate of flow of heat for any substance.

The application of this method of making a mechanical balance of the heat effects of varying temperatures has resulted in the apparent establishment of the real constancy of the relative coefficients of expansion between Bailey's metal, Jessup's steel, and Chance & Son's glass between the limits of —5° and +95° Fahrenheit. 293 sets of comparisons have been made, extending from Dec. 18, 1886, to July 14, 1887. About one-half of this number were made from one-quarter to three-quarters of an hour after sunrise on clear days. Perhaps one-eighth of the entire number were made on days when there was a very steady temperature throughout the entire day, and it is to be especially noted that the mean of the entire series of balanced values is nearly the same as the mean of the values derived from the observations on rainy days.

The limitation of the time of closing the series was made with a definite object in view. Until all but the last two series of observations had been completed, viz: from —5° to +65°, I had no knowledge of the results to be expected. In May of the present year, the observations made up to this point were reduced, and I then knew, for the first time, that the laws of constancy was likely to be established. *As the remaining observations went forward I found it impossible to resist the tendency to obtain a result which I expected.* This experience presents a curious psychological question. When any mean value has been really established, there must be after that as many positive as negative corrections, if the observations are continued for a sufficient length of time, and one can obtain, within narrow limits, almost any value

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desired by *stopping the series of observations at the right time.* In order to obtain, as far as possible, freedom from errors of this class, it was decided, early in June, to close the series on July 14, 1887, and this programme was strictly carried out.

The plan of the work adopted is an exceedingly simple one. It consists:

(a) In obtaining a sufficient number of observations in groups at intervals of about 10° Fahrenheit, in order to neutralize the accidental errors of observations in that group.

(b) In either limiting the comparisons to the times when equilibrium appeared to be established between the temperature of the bars of metal, of the thermometers and of the surrounding air, or in distributing the observations symmetrically about the point of no variation of temperature as determined by thermometers placed upon the upper surface of the bars of metal and also supported in the air in nearly the same horizontal plane.

(c) In making continuous comparisons when the atmospheric conditions were such that a steady temperature could be maintained for several hours at one time as on rainy days. It has been found that the comparisons could be safely continued for an entire day, at intervals of one hour, when the entire rise or fall of temperature did not exceed 0.8° Fahr. Since the general tendency of the temperature would, on such occasions be upward, every opportunity possible was chosen in which the slow drift of the temperature was in the opposite direction. Cloudy days following a change of wind to the north during the previous night, were utilized as far as possible, but it was occasionally found necessary, in order to maintain an equality between the number of upward and downward drifts of temperature, to resort to heating the comparing room by convection for several days, and then to allow a very gradual dissipation of the heat thus stored up.

During this series of observations the comparator was mounted in the third story of one of the College buildings. Contrary to expectation, this room was found to be far bet-

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ter adapted to the control of the temperature than the room beneath the rotunda of the Observatory at Cambridge. The comparator is mounted in a direct line between two large windows, so that it is possible to obtain, practically, open air exposure. The dimensions of the room are 20×12×12 feet. On account of the great thickness of the brick walls and the thoroughness with which the outside air can be excluded by means of heavy shutters which slide into openings in the centre of the walls it is easy to control the range of temperature within narrow limits for any selected time.

While at Cambridge the whole variation, within a given time, was less than at Waterville, it was far more difficult to ascertain, at the former place, the precise point of no variation of temperature, and hence there was far greater danger that the systematic errors in question would escape detection.

The stop method was employed in all of the observations. The stops having been firmly clamped at a distance apart of approximately one yard, the microscope carriage was first brought into contact with the left-hand stop, and two readings of the microscope for coincidence between the micrometer line and the initial line of the standard were taken. The carriage was then brought into contact with the right-hand stop, and four readings for coincidence were taken. As a check upon the constancy of the parts of the apparatus involved in the entire forward and backward movement of the carriage which carries the first microscope, two readings were again taken for the first contact. It is evident that the difference between the readings will give the length of the bar with respect to the distance between the stops, and also that when the difference between this value and the corresponding value for another standard is taken, the distance between the stops will be eliminated.

The following example will serve as an illustration:

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R_2 with stops.

Left.	Right.	R—L.
7 rev. 42.1 div.	7 rev. 40.8 div.	—1.3 div.

R_3 with stops.

Left^1.	Right^1.	R^1—L^1.
7 rev. 37.8 div.	7 rev. 35.0 div.	—2.8 div.

When R—L or R^1—L^1 has the minus sign the interpretation is that the length of the standard is greater than the distance between the stops. In this example yard R_2 is 1.3 divisions and yard R_3 is 2.8 divisions longer than the distance between the stops.

If we subtract R—L for R_2 from R^1—L^1 for R_3 we have:

$$(R^1—L^1)—(R—L)=R_2—R_3.$$

In this case $R_3—1.5 \text{ div.}=R_2$

indicating that R_3 is 1.5 divisions longer than R_2.

The results of the comparisons between R_2, R_3 and G are given in exhibit (E). The glass bar G has five separate yards laid off upon its surface, each being the result of corrections derived from the one previously ruled. Yard G_1 has lines which have a width of about 5 mikrons, while the width of the lines of G_3 is but little over 1 mikron. Both G_1 and G_5 were used in all the comparisons in order to ascertain whether any personal equation exists which depends on the size of the lines.

The results of the comparisons are expressed in divisions of the micrometer in which

$$1 \text{ division}=0.567\mu.$$

In the discussion of the observations the mean result for each group of ten sets is compared with the result from the next succeeding group, as shown in exhibit F. The separate results for the value of the relative coefficients, expressed in mikrons, are as follows:

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Limits of temperature.			Relative Coefficients. $(R_2—R_3)$	$(R_2—G_1)$	$(R_2—G_5)$
4.48°	15.53°		3.53μ	4.70μ	4.77μ
15.53	26.05		3.52	4.64	4.54
26.05	35.54		3.48	4.50	4.62
35.54	45.18		3.52	4.31	4.28
45.18	54.52		3.58	4.65	4.65
54.52	64.94		3.55	4.77	4.78
64.94	75.21		3.53	4.69	4.65
75.21	85.09		3.38	4.48	4.40
		Means.	3.51μ	4.59μ	4.59μ

Three facts will at once arrest the attention upon an examination of the results in this table.

(1) There is no well defined evidence of an increase in the value of the relative coefficients, corresponding to an increase in the temperature.

(2) There is an exact agreement between the values of the coefficients derived from G_1 and from G_5.

(3) The probable errors of observation are much greater for the glass bar than for the steel bar. This is due to the greater mass of the glass bar and to the consequent lack of the precision with which the time of equilibrium can be determined.

In exhibit (G) the comparisons are arranged in groups of 10 observations each. In the first half of the series the residuals $R_2—R_3$ and $R_2—G$ are reduced to the mean temperature 33.86° with the relative coefficient first obtained, viz: 6.17 div. for $R_2—R_3$ and 8.09 div. for $R_3—G$. In the second half the reduction is made to the mean temperature 73.84°. The solution by Least Squares of the equations of condition found from these observations, gives the following values for the absolute coefficients:

	R_3	G_1	G_2
First half of series	5.213μ	4.189μ	4.179μ
Second half of series	5.278	4.153	4.178

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While the result for R_3 shows a slight increase for an increase in the value of the temperature, the glass bar indicates a corresponding decrease. The agreement here indicated is greater than we ought to expect except when the number of comparisons is very large.

In exhibit (H) all of the observations are reduced to 62°.0. The solution of the equations gives:

For R_3 coefficient $= 5.205\mu$
" G_1 " " $= 4.126\mu$
" G_5 " " $= 4.127\mu$

When these values are reduced to the corresponding values for the centigrade scale we have the following results, which may be compared with the results published before this series was commenced in the Proceedings of this Society for 1885, page 157, and in the Proceedings of the American Academy of Arts and Sciences for 1882–3, page 385.

	From the present series.	From previous observations.
Coeff. of R_3	$= 10.250\mu$	$= 10.246\mu$
Coeff. of $\frac{G_1+G_5}{2}$	$= 7.428$	$= 7.400$

It appears also from this solution that yard R_3 is 9.78 divisions or 5.55μ longer than R_2 and that G_1 is 1.07 divisions or 0.61μ longer than R_2 at 62°.0 Fahr. In order to compare these values with those previously obtained it will be necessary to apply the corrections due to the horizontal curvature of the ways of the comparator. It will be remembered that the standards were arranged side by side upon the comparator. The distance from R_2 to R_3 was 19.7 mm. and from R_3 to G was 53.4 mm. The correction for the horizontal curvature is 5.40 divisions for each centimeter of distance from the central line between the ways

We have therefore:

$R_2 - R_3 = -9.8$ div. $+ 10.6$ div. $= + 0.8$ div. $= + 0.5\mu$.
$R_2 - G_1 = -1.1$ div. $+ 28.8$ div. $= +27.7$ div. $= +16.7\mu$.

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By the reference already given it will be found that the corresponding values previously found are:

$$R_2 - R_3 = -0.5\mu$$
$$R_2 - G_1 = +17.0\mu$$

It is therefore evident that no change greater than the accidental error of observation has taken place in the lengths of these standards since they were first made.

We are now prepared to make a practical application of the results of our inquiry. Since we can assume the relative coefficients of expansion between the bronze and the steel bars to be 6.20 divisions of the micrometer for each and for every degree of the Fahrenheit scale and of the bronze and glass bars to be 8.11 divisions, we can ascertain the corrections of the thermometers employed to measure the very low temperatures on Jan. 4, Jan. 9 and Jan. 19, 1887. The following observed relations between the lengths of R_2, R_3, G_1 and G_5 are placed beside the values compiled from the data given in exhibit E.

Date.	Thermometer.	Observed.	R_2-R_3 computed.	c—o
Jan. 4.	—19.80°	—490.0 div.	—517.0	—27.0
9.	—13.90	—457.0	—480.8	—23.8
19.	—11.85	—459.6	—467.7	— 8.1

Date.	Thermometer.	Observed.	R_2-G_1 computed.	c—o
Jan. 4.	—19.80°	—645.8 div.	—683.1	—37.5
9.	—13.90	—614.0	—635.2	—21.2
19.	—11.85	—614.0	—618.6	—

Date.	Thermometer.	Observed.	R_2-G_5 computed.	c—o
Jan. 4.	—19.80°	—623.1 div.	—658.8	—35.7
9.	—13.90	—596.3	—616.6	—20.3
19.	—11.85	—588.9	—600.0	—11.1

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If we assume the computed values given above to be real, they may be reduced to the observed values by dimnishing the residuals c—o by the quotient of c—o divided by 6.20 for the steel bar, and by the the quotient of c—o divided by 8.11 for the glass bar. We have therefore:

	From $R_2—R_3$	From $R_2—G_1$	From $R_2—G_5$	Means.
Correction at —19°.8	= +4°.36	+4°.60	+4°.40	+4°.34
Correction at —13.9	= +3.84	+2.61	+2.50	+2.98
Correction at —11.85	= +1.31	+0.56	+1.37	+1.08

These results confirm the conclusion reached by an entirely different method of investigation, undertaken by the writer, viz: *That mercurial thermometers, as a class, read too low when the temperature is below minus ten degrees Fahrenheit.*

It must be noted, however, that these observations were made with great difficulty, and definite conclusions can hardly be drawn from them.

ADDENDUM.

I.

The values $R_2—R_3 = —9.8$ div. and $R_2—G_1 = —1.1$ div. were obtained from the solution of the equations of condition about ten days before this address was delivered. They were, therefore, unknown to me when the observations were made. Inasmuch as the comparison of any subsequent determinations with these values would offer additional evidence in regard to the correctness of the theoretical considerations offered in this paper, it was thought worth while to make an additional series of comparisons after my return to Waterville after the close of the annual meeting. The entire data for this series are recorded in Exhibit (K). The original readings for $R_2—R_3$, $R_2—G_1$, are given because they furnish the means of comparison with the nearly constant

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value of the distance between the stops of the comparator. The corrected readings of the thermometers are given in columns 2, 3, 4 and 5. No. 1 rested upon R_3, and No. 2 upon G. No. 3 rested upon the edge of a paper box, at about the same height as G, while No. 4 was similarly supported at the height of R_2. In the observations of October 4th, 5th and 6th, a Green standard was inserted in a groove cut in a bronze bar having the same cross-section as R_2. The depth of the groove is half an inch, and the bulb of the thermometer was immersed in mercury. The opening was covered by a thin sheet of rubber placed upon the surface upon which another bar was placed.

All the observations are reduced 62.0° by means of the relative coefficients 6.2 div. and 8.11 div. The last two columns contain the residuals $[R_2 - R_3] + 9.8$ div. and $[R_2 - G_1] + 1.1$ div.

An asterisk is placed opposite the comparisons which are too far removed from the point of no variation of temperature to be safely included in the determination of the relations between R_2 and R_3, and between R_2 and G_1.

The results of the observation which have been made since September and which have been arranged with reference to a more complete test of the laws relating to the action of heat upon the metals under investigation, enunciated in this paper, than was possible in the earlier comparisons, in which the only object aimed at was an equal and symmetrical distribution with respect to upward and downward *drifts* of temperature, will be recorded in the form of conclusions with the references to the data by which they are established.

It may be well to restate, at this point, the grounds upon which preference is given to air-contact observations, in the light of more recent experience.

(a) In order that the expansion and contraction of metals under variations of temperature may take place under the action of forces for which simple analytical expressions

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may be obtained, it is essential that the medium by which the heat is communicated to them shall be symmetrically distributed at all points of their surfaces. This condition is indeed fulfilled to a certain extent in liquid contacts, but by no means as perfectly as in air contacts.

(b) The temperature can be kept under far better control in air contacts than in liquid contacts.

(c) As will be presently shown, the air seems to have very little power to communicate to the metals under comparison, the generally higher temperature of the observer. Without doubt the temperature of the air immediately surrounding the observer is raised, but it is not certain that the currents created reach the metals directly. The observations of and Mallet in the ascent of the "Horla" tend to show that currents of air do not readily mingle when they meet, and that colder currents pass under warmer ones instead of mingling with them. Mass, however, is the chief factor of resistance to change in length.

(d) The greater part of the conductive effect in liquid contacts is indeed nearly instantaneous in its action, and metals submerged in liquids quickly acquire the temperature of the immediate layers by which they are surrounded, but the effect of changes in temperature upon the liquid is far more irregular and requires a much longer time for complete saturation than in the case of metals when submitted to the same conditions of temperature. Metals probably change their temperature under the action of a single force. Liquids pass from one temperature to another under the action of at least two forces, viz.: by direct transfer as in the case of metals and under the action of currents. My own observations seem to show that while these currents are at first more marked in the case of water than in the case of mercury the difference is hardly appreciable at the expiration of two hours after the removal from a given constant temperature to a different temperature also constant, but the time required for each to reach complete saturation is nearly the same,

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viz., from eight to twelve hours. This limit of time is approximately independent of the quantity of the liquids used in the experiment. The ordinary method of maintaining liquids at an apparently constant temperature either by a gentle flow or by vigorous stirring, creates an additional source of error by adding a third force to the two already mentioned, viz., an increase of temperature by friction.

(e) Since a liquid must under ordinary circumstances have a part of its surface exposed to the air, the condition named under (a) must always be violated in the passage of the liquid from one temperature to another.

The following are the conclusions drawn from the observations given in detail under exhibits K, and L.

The limit of error may be safely placed at about 1.0μ. It is doubtful whether two equally careful observers will uniformly obtain results which will show an agreement much closer than this limit; for example, in the comparison of two meters. The errors due to personal equation, and especially to personal equation in focus, are of themselves quite sufficient to demand at least the limit of 1.0μ, even if the graduated surface remains in an invariable plane when the bar of metal is supported at two fixed points. The limit of the relative error in repeated comparisons under the same conditions, may be placed at about 0.2μ.

(1) *In the comparisons of standards of length which have considerable mass, the presence of the person of the observer will produec no sensible effect in increasing their length within the limits of the accidental errors of observation, during the time ordinarily required for a complete comparison.*

The limit of safety may be extended to two hours of continuous work when the air is comparatively free from moisture. If the time required for a single comparison does not exceed five or six minutes the observations may be safely repeated at intervals of one hour without limit in regard to time. An examination of the comparisons between R_2 and R_8, for October 27, exhibit K, will show that under a nearly

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constant temperature, the normal relation between these standards was maintained between 6^h 10^m and 8^h 25^m. Until 7^h 20^m the interval between the comparisons was about eight minutes. Those made between the limits, 7^h 18^m and 10^h 45^m on September 29, between 6^h 20^m and 9^h 0^m on October 3, between 1^h 45^m P. M. and 3^h 45^m on October 5, between 6^h 30^m A. M. and 4^h 0^m P. M. on October 10, and between 3^h 0^m P. M. and 5^h 10^m on October 16, indicate the maintainance of nearly normal relations between the standards compared. This date comes from the table which you have.

The statement here made is so greatly at variance with the ordinary view, that I have considered it important to verify it by a large number of special experiments, an account of which is necessarily omitted here. Any investigator can verify it by making continuous comparisons on stormy days on which a nearly constant temperature is maintained. It is essential, in these experiments, that the observer shall not touch the bars compared. It is also essential that the comparing room shall have dimensions of considerable extent, especially in height. The latter dimension should never be less than 12 or 15 feet.

(2) *When contact takes place through the hands between the person of the observer and the bars of metal compared, a nearly instantaneous increase in length takes place which diminishes in a slightly decreasing ratio as long as the contact is continued, but which ceases instantly when the contact is broken.*

The observations of Septcmber 29, Exhibit K, and of October 2 and 3, furnish an illustration of the facts, first, that the increments produced by successive charges of heat through contact between the hand and the three bars of metal are nearly equal; second, that there is neither an increase nor a decrease in length during the five minutes immediately following the charge; and, third, that the increase is nearly the same in amount for the steel, the bronze and the glass bars. The time required for the complete dissipation of the heat stored in the bars in this manner is nearly a constant, namely about 3 hours.

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Many observations have been made, showing that equal increments in length correspond nearly with equal increments in time, at least for moderate intervals of time. Thus, in the observations of October 2 the total increase in length after four charges, made at intervals of five minutes, is the same as three times the increment in length due to the first charge.

Substantially the same results were obtained by placing a rectangular block of iron having a temperature of about 300° in contact with one of the vertical faces of the bar of metal. It should be noted that contact through the hands repeated at short intervals furnish a constant supply of heat only for a short time.

(3) *Whenever a change of temperature occurs, the effect of the change is indicated by thermometers which rest horizontally upon the upper surface of the bar of metal, in advance of the actual change in length.* It follows, therefore, that if the observations are reduced to an assumed higher temperature by means of the known coefficient of expansion, the computed length will be inevitably too short for a rising temperature, and too long for a falling temperature.

It is just here that we must look for an explanation of the anomalies which have given so much trouble. According to present indications metals *do not* "crawl" through changes in their molecular structure. For a rising temperature the change in length is not as rapid as the corresponding change in the indications of the thermometer with which the temperature of the metal is supposed to be measured. The strongest emphasis must be placed upon the fact that increments in length will *never* overtake those given by the thermometers, as long as the increase of temperature is continuous, even when it does not amount to more than one or two-tenths of a degree per hour.

(4) *The amount by which the indicated reading of the thermometer fails to indicate the real temperature of the standards compared, varies with their mass, and their shape, being less for*

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a larger surface area, and greater for a large mass having equal dimensions in cross-section.

The last two columns in Exhibits K and L represent the relative lengths of the bronze bar R_2, the steel bar R_8 and the glass bar G, at 62.0° Fahr., after the corrections for absolute error in length at this point have been applied. A slight examination will show that the systematic character of the relations given is very marked. They commence to be exhibited the moment a change of temperature is noted by thermometers Nos. 1 and 2, which rest horizontally upon the surface of R_2 and of G respectively. It will be seen, also, that the discordances between R_8 and G are much greater than those between R_2 and R_3, the mass of G being more than three times as great as that of R_3.

(5) *The systematic errors in question practically disappear whenever the readings of the thermometers placed upon the surface of the bars, remain in agreement with the indications of a thermometer supported in air at the same hight as the upper surface of the bars of metal for a period of three hours.*

In other words, these systematic deviations disappear when equilibrium occurs between the temperature of the upper surface of the bars compared, of the thermometers employed and of the surrounding air.

The determination of the exact time of equilibrinm is facilitated by the comparison between the two thermometers which rest upon the bronze and the glass bars. For a rising temperature the thermometer upon the glass bar reads from 0° to 1.0° higher than that upon the bronze and the steel bars, while for a falling temperature it reads from 0° to 1.0° lower than the other thermometers. The agreement between the readings of these thermometers is therefore an evidence that equilibrium has been reached. That this variation is not due to reflection from the smooth surface of the glass, is shown by the fact that the same phenomenon is seen when a glass bar having a sand-surface finish is substitutud for G. That the question of mass is not involved, is shown by the

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fact that there is no change in the observed relation when a glass bar having the same mass as R_2 is substituted for G. All indications point to the conclusion that the variation here noted is due to the greater absorptive and emissive power of the glass.

(6) *The danger of the introduction of systematic errors of the class indicated is much greater for slow than for rapid changes of temperature, and is also much greater when the direction of the flow changes from plus to minus than when it is continuous in the same direction.*

This conclusion is clearly confirmed by my experience with the new comparing room beneath the rotunda of the Observatory at Cambridge. It was found impossible to guage the delivery of the heat and the cold stored up in the immense masses of stone with which the comparing room was surrounded.

The comparing room of the International Bureau at Breteuil is for most purposes incomparably the best in the world, but it must be said that danger of the introduction of systematic errors is always present on account of the great length of time required to pass from one temperature to another, Even the immense mass of the comparator may itself become a reservoir of heat and cold. In fact, it seems to be almost impossible to avoid errors of this class for extremely slow changes of temperature. This danger relates only to comparisons between standards of very small and of very large mass. The present form of the prototype standards is so admirably adapted to take the temperature quickly that there is little danger in the direction indicated when standards of the same class are compared.

It is this slow but sure movement of the molecules of matter in the metal employed, a movement far too subtle to be studied in detail with the aid of the most delicate thermometer, but whose integration can be accurately measured with the aid of the microscope, which produces the apparent variations in length between

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comparisons made at differene times of the day and especially at different seasons of the year. The conditions under which equilibrium is determined vary with the rapidity of the increase or of the decrease of the temperature, and hence, they vary also with the season of the year. It hardly needs to be added, that these conditions must be determined for the particular location of the comparator with which the comparisons are made. At Waterville the time required for the equilibrium to be established is about three hours, but in the under-ground comparing room at Cambridge equilibrium was often delayed many hours, and in some cases, probably for several days after the indicated agreement between the temperature of the graduated surfaces of the bars and the surrounding air.

(7) *Admitting the observed fact that the thermometer placed upon the surface of the bars compared, always gives a wrong indication except at the moment of real equilibrium, it follows that the constant tendency of obsevers is to obtain for the length of a given standard at an assumed higher temperature, a result which is too small, since the comparisons are ordinarily made without forethought under a rising temperature.* The time between 10^h A. M. and 3^h P. M. is the most unfavorable which can be selected. So also the inevitable tendency is to make the derived length relatively shorter in summer than in winter.

The observations of November 3, exhibit L, furnish a good illustration of the effect of both slow and of rapid changes of temperature. During the night of November 2, the temperature of the comparing room and also of the room adjoining was kept at about 85°, until two o'clock on the morning of November 3, when it was allowed to diminish gradually. At the time of the first observation at $6^h\ 30^m$, the computed relation between R_2 and R_3, made R_3 13 divisions of the micrometer shorter than R_2. Until ten o'clock when equilibrium was apparently established between R_2, R_3 and the surrounding air, the deviation gradually diminished in value when the normal relation between R_2 and R_3 was estab-

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lished. In the case of R_2 and G the deviation was much more rapid, but the normal relation was not reached until half an hour later. At $12^h\ 15^m$ the shutters of the four windows in the upper part of the partition between the two rooms were opened and a current of air having a temperature of about 95° was allowed to flow through until $1^h\ 50^m$ P. M. when the windows were closed. At about 5^h the surface equilibrium between R_2 and R_3 was established, while for R_2 and G it occurred near 6^h. During the night, under a slowly falling temperature, a complete reversal of the indications took place. The stationary period for R_2—R_3 was reached at about $11^h\ 30^m$, while for R_2—G it had not been reached at midnight.

(8) *The objection which will without doubt be made, that the temperature should be determined with thermometers placed in wells sunk in the bars of metal employed and surrounded with mercury, instead of being placed horizontally upon the upper surface, is not a valid one.*

The practice of placing thermometers in wells in a vertical position is indefensible, because the stems are exposed to contact with layers of air at different heights. There is less objection to the submergence of the thermometer in mercury at or near the centre of the bar, but unless the air is excluded from the groove, there will be very little gain in this arrangement. A thermometer mounted in this manner fails entirely, however, to indicate the outward flow of the heat from the surface. It is, indeed, true that under rapid changes of temperature the immersed thermometer will, at least at first, correspond more nearly with the internal temperature of the bar than the one which rests upon the surface, but for all slow changes, and especially when changes in the direction of the flow occur, the danger of the introduction of systematic errors is greater than for surface exposure.

According to present indications the real temperature of the entire mass of the metal will be obtained by taking the

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arithmetical mean of the readings of the temperature at the surface and at the center part of the bar of metal.

(9) *Whenever a constant temperature can be maintained for three and one-half hours, the point of complete saturation will be so nearly reached that the microscope will fail to detect variations in the length of the standards compared.*

Of course this remark relates to bars of metal which have considerable mass. For thin bars the time required is much less. When eight or ten comparisons are made under this condition, the result obtained will be found to have as much value as a much larger number of comparisons made at equi-distant intervals from the critical period of quiesence of temperature. The mean of the results of two or three day's work under these favorable conditions will give a near approximation to the real relation between the standards compared.

(10) *The apparent stationary point as indicated by the reading of the surface thermometer can be regarded as the real point of equilibrium only when the thermometric indications have remained constant for at least three hours.* In other words, we must know the temperature conditions which have existed during the three hours immediately preceding before we can assume that the real point of equilibrium has been reached. The observance of this precaution is of the utmost importance, since for a rising temperature it will be inevitable that a wrong relation between the standards will be determined for every observation between the time when the reading of the surface thermometer becomes stationary and the time elapsed before the two standards reach the normal relation for that constant temperature. In a general way the reverse action takes place when the temperature is derived from the thermometer placed at the center of the standard bar. If the temperature is determined by taking the mean of the thermometer readings at the surface and at the interior of the the bar, a near approximation to the mean temperature will be obtained.

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(11) *The error in the determination of the normal relation between the standards, due to the more sluggish action in the case of the metals, will not be elimnated as long as there is a movement of the temperature in the same direction if either the surface or the interior temperature be taken alone, but will be nearly eliminated when the mean of the two indications is taken.*

On the other hand, the elimination will take place, more or less perfectly, in the combination of observations, having nearly equal upward and downward drifts of temperature. It seems to be more exactly secured in those cases in which there is a gradual rise or fall from equilibrium. More observations are needed to determine the law under which the elimination takes place. This elimination will ordinarily be more perfect for surface indications than for those at the interior of the standard bar.

(12) *After every precaution has been taken, instances sometimes occur in which an entire series of observations yield results which differ persistently from the normal relation.*

The amount of this deviation rarely exceeds 1μ. It is possible that the explanation of this result may be found in the conclusion given under division (11), but I have been thus far inclined to attribute it to variations in the humidity of the atmosphere.

(13) *The time required to pass from complete saturation at one temperature to complete saturation at another temperature is nearly a constant.*

In the case of the three metals under consideration this time does not vary much from three and one-half hours, the time for the glass bar being a little less than that for the bars of bronze and steel. This conclusion is verified by all of the thirteen cases given under exhibit M.

The accurate determination of this time will be possible only when the means are at hand for a better control of the temperature than are at present available. The effect of a change of the temperature of the surrounding air in the same direction as that which is taking place in the bar under ob-

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servation will be apparently to shorten the time required for complete saturation, while that in the opposite direction will apparently lengthen the time.

The time required to pass from complete saturation at 0° to complete saturation at 1° is nearly the same as the time required to pass from complete saturation between the limits 0° and 100°.

This law has been found to hold nearly true as far as T=30°, and it probably holds approximately true for any value of T whatever.

The law may be stated in another form as follows: Two bars of metal are allowed to remain in a room having a given constant temperature until complete saturation has taken place. One of them is removed to a room in which a temperature, one degree higher than in the first room, is maintained. The other is removed to a room in which a temperature e. g. 30° higher is maintained. Then each bar will take the temperature of the medium in which it is placed in the same time, viz. in about three and one-half hours.

(15) *It follows as a consequence of the law given under division (14) that the relative specific heat of metals may be obtained by comparing the times required for complete saturation if the bars of metal compared have the same mass and the same shape, since the change in length is a measure of the total amount of the work done by the heat which has been absorbed by the metal.*

(16) *The law under which changes of length take place in bars of metal under the action of a constant supply of heat may be expressed by a series under "Taylors Theorem" in terms of the ascending powers of the time.*

Let F = the total change in length between the limits t_0 and t.

Let F_0 = the correction in length at the instant t_0.

Let $\frac{df}{dt}$ = the rate of change during the instant d t.

Then $F=F_0+\frac{df}{dt}t+\frac{1}{2}\frac{d^2f}{dt^2}t^2+\frac{1}{6}\frac{d^3f}{dt^3}t^3+\frac{1}{24}\frac{d^4f}{dt^4}t^4+$&c.

The values of $\frac{df}{dt}$, $\frac{d^2f}{dt^2}$, $\frac{d^3f}{dt^3}$, &c. can be determined from

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the series of differences between the changes in length which occur in any assumed equal intervals of time by means of the following formulas. [See paper by Miss Anna Winlock, in the Memoirs of the American Academy of Arts and Sciences, Vol. XI, Part IX, page 248.]

Let w=any interval of time whatever.

Let Δ_1 Δ_2 Δ_3 &c.=the successive orders of differences in the given series of comparisons.

Let Δ^{I} Δ^{II} Δ^{III} &c.=the differences opposite the initial functions.

Then $\Delta_1 = \frac{\Delta_1^{+\frac{1}{2}} + \Delta_1^{-\frac{1}{2}}}{2}$, $\Delta^{III} = \frac{\Delta_2^{+\frac{1}{2}} + \Delta_2^{-\frac{1}{2}}}{2}$ &c.

$\Delta^{II} = \Delta_2$ $\Delta^{III} = \Delta_3$, &c.

$$\frac{df}{dt} = \frac{1}{w}\left[\Delta^{I} - \tfrac{1}{6}\Delta^{III} + \tfrac{1}{30}\Delta^{V} - \tfrac{1}{140}\Delta^{VII} + \tfrac{1}{630}\Delta^{IX} - \&c.\right]$$

$$\frac{d^2f}{dt^2} = \frac{1}{w}\left[\Delta^{II} - \tfrac{1}{12}\Delta^{IV} + \tfrac{1}{90}\Delta^{VI} - \tfrac{1}{560}\Delta^{VIII} + \tfrac{1}{3150}\Delta^{X} - \&c.\right]$$

$$\frac{d^3f}{dt^3} = \frac{1}{w^2}\left[\Delta^{III} - \tfrac{1}{4}\Delta^{V} + \tfrac{7}{120}\Delta^{VII} - \tfrac{41}{3024}\Delta^{IX} + \&c.\right]$$

$$\frac{d^4f}{dt^4} = \frac{1}{w^4}\left[\Delta^{IV} - \tfrac{1}{6}\Delta^{VI} + \tfrac{7}{240}\Delta^{VIII} - \tfrac{41}{7560}\Delta^{X} + \&c.\right]$$

$$\frac{d^5f}{dt^5} = \frac{1}{w^5}\left[\Delta^{V} - \tfrac{1}{8}\Delta^{VII} + \tfrac{13}{1440}\Delta^{IX} - \&c.\right]$$

$$\frac{d^6f}{dt^6} = \frac{1}{w^6}\left[\Delta^{VI} - \tfrac{1}{4}\Delta^{VIII} + \tfrac{13}{2240}\Delta^{X} - \&c.\right]$$

$$\frac{d^7f}{dt^7} = \frac{1}{w^7}\left[\Delta^{VII} - \tfrac{5}{12}\Delta^{IX} + \&c.\right]$$

$$\frac{d^8f}{dt^8} = \frac{1}{w^8}\left[\Delta^{VIII} - \tfrac{1}{8}\Delta^{X} + \&c.\right]$$

$$\frac{d^9f}{dt^9} = \frac{1}{w^9}\left[\Delta^{IX} - \&c.\right]$$

$$\frac{d^{10}f}{dt^{10}} = \frac{1}{w^{10}}\left[\Delta^{X} - \&c.\right]$$

The following numerical equations result from the data given in experiments 1 to 12 under exhibit M. It will be understood that they represent only an approximation to

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the true values of the coefficients on account of the imperfect character of the data, due to the partial failure to maintain a constant temperature during the comparisons. They will, however, serve the present purpose as examples of this method of proceedure.

The values of the coefficients, which are expressed in the form of logarithms, are derived from the differences opposite the middle point of each series, viz: along the line of the dashes. For a wide range of temperature, it was found necessary to extend the observations sufficiently far to make the intervals equal to 48 minutes.

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No. of Experiments.	Name of Standard.	Range of Temperature.	Equations.					
1	R_2	34.05 to 64.1	F = — 88.7 div.	$-9.95646t$	$+7.51600t^2$	$-5.22540t^3$	$+3.26712t^4$	$-0.89465t^5$	$-8.46347t^6$
2	R_2	32.05 to 50.1	F = — 27.3	$-9.63383t$	$+7.39924t^2$	$-4.92046t^3$	$+2.84584t^4$	$-0.45414t^5$	$+6.65729t^6$
3	R_2	59.05 to 56.8	F = + 43.2	$+1.12985t$	$-1.42516t^2$	$+1.01019t^3$	$+1.54046t^4$	$-0.76882t^5$	$-1.33984t^6$
4	R_2	68.0 to 64.1	F = —165.2	$+8.74688t$	$+6.22015t^2$	$+5.28043t^3$	$-3.56767t^4$	$-0.54120t^5$	$-9.21214t^6$
5	R_3	34.05 to 64.1	F = —116.5	$-9.64849t$	$+7.29454t^2$	$-5.05536t^3$	$+3.02783t^4$	$-0.02650t^5$	$-8.05955t^6$
6	R_3	32.05 to 49.7	F = —126.7	$-9.40041t$	$+7.12375t^2$	$-5.00743t^3$	$+3.93092t^4$	$-9.41771t^5$	$-8.12711t^6$
7	R_3	59.05 to 56.6	F = — 2.4	$+9.23430t$	$-7.04299t^2$	$-5.21995t^3$	$+2.51125t^4$	$+1.21789t^5$	$-8.50971t^6$
8	R^3	68.0 to 64.1	F = —158.4	$+8.72657t$	$+6.52890t^2$	$+4.64216t^3$	$-3.47995t^4$	$+9.57116t^5$	$+9.26711t^6$
9	G	34.05 to 63.6	F = —126.7	$-9.22912t$	$+7.27820t^2$	$-5.05174t^3$	$+2.47079t^4$	$-0.43310t^5$	$+8.23707t^6$
10	G	35.5 to 50.1	F = —143.4	$-9.25540t$	$+7.35942t^2$	$-5.50141t^3$	$+3.39968t^4$	$+9.57116t^5$	$-8.74524t^6$
11	G	68.0 to 63.0	F = —135.4	$+8.88843t$	$-7.25037t^2$	$+4.56674t^3$	$+3.09055t^4$	$+0.09404t^5$	$-8.79299t^6$
12	G	32.0 to 65.0	F = +251.7	$+9.68590t$	$-7.64267t^2$	$+5.09645t^3$	$-3.39461t^4$	$+1.55524t^5$	$-9.05920t^6$

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That these eqations represent with a close approximation all the observations of a given series is proved by the agreement of the computed with the observed values of F which were not employed in the formation of the equations. The extent of the agreement is shown in the following table. The residuals (c—o) are not given from the data represented by the equations since these results must, from the nature of the solution of the problem, be zero, as in fact they are:

Experiment 1.

Time h m	(c—o)
7 51	+4.9 div.
8 7	+1.8
8 39	+1.3
8 55	+0.0
9 27	—0.8
9 43	—1.2
10 15	+0.5
10 31	+1.9
11 3	—2.0
11 41	+0.7

Experiment 2.

Time h m	(c—o)
10 16	—0.1 div.
10 32	+0.0
11 4	—0.6
11 20	—0.4
11 52	—1.1
12 8	—1.8
12 40	—2.6
12 58	+0.2
1 30	+1.5
1 46	+1.5

Experiment 5.

Time h m	(c—o)
7 55	+2.1 div.
8 11	+0.5
8 43	+0.7
8 59	+2.1
9 31	—0.9
9 47	—1.4
10 19	—0.8
10 35	+0.9
11 7	+2.0
11 23	+0.6

Experiment 6.

Time h m	(c—o)
10 6	+1.3 div
10 22	—0.3
10 54	+1.5
11 10	+1.9
11 42	—1.9
11 58	—1.6
12 30	+0.0
12 46	—0.1
1 18	+0.9
1 34	+2.2

Experiment 9.

Time h m	(c—o)
7 47	+3.6 div.
8 3	+3.0
8 35	—0.9
8 51	—0.6
9 23	—1.1
9 39	—0.9
10 11	+0.9
10 27	—0.6
10 59	+1.1
11 15	+0.5
12 45	—0.4
1 1	—0.7

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Experiment 3. Experiment 7. Experiment 10. Experiment 12.

Time h m	(c—o)	Time h m	(c—o)	Time h m	(c—o)	Time h m	(c—o)
2 31	—1.0 div.	2 24	—1.8 div.	2 52	+1.5 div.	10 12	+4.5 div.
3 3	+0.4	2 56	—0.9	3 22	+0.1	10 44	—0.2
3 35	+0.4	3 28	+0.3	3 56	—1.3	11 16	+0.9
4 7	+0.1	4 0	+0.7	4 26	+2.5	11 48	+0.1
4 39	—1.0	4 32	+0.8	4 58	+0.5	12 20	+0.9
5 11	+1.3	5 4	—3.1			12 52	—0.9

Experiment 4. Experiment 8. Experiment 11.

Time h m	(c—o)	Time h m	(c—o)	Time h m	(c—o)
2 57	+0.0 div.	3 1	+0.3 div.	2 53	+0.9 div.
3 29	—0.3	3 33	+0.1	3 25	+0.5

It will be seen, therefore, that *all* of the equations are verified within the limits of the accidental errors of observation.

We have a second test in the agreement of the value of the observed change of length between the time when the standard was brought into the comparing room and at the conclusion of the series, with the computed value of the change obtained by multiplying the coefficient of expansion of the standard under observation by the difference of the temperature at the beginning and the end of the series. The computed value of the relation between the length of the yard and the distance between the stops obtained from the equation at the initial time is, however, subject to much uncertainty in these observations, since in the removal of the standards it was necessary to carry them a considerable distance through two intervening rooms. The rapidity of the change at the initial time of observation will also prevent a very accurate determination of the length at that instant, neither can the computation from the equation be extended to a time much earlier than that of the first comparison. We may take experiment 4 as an example. The computed value of F at $9^h\ 27^m$ is 220.4 div. The total change in

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length during the interval of $3^h\ 27^m$ was 269.6 div. The absolute coefficient of standard R_2 is 16.83 divisions for each degree Fahr. Hence the total change equals [16.83 div.] [50.°10—32.°05]=303.8 div. The difference, 34.2 div., is perhaps as small as one should expect in the present case.

It is frequently desirable to obtain an expression for the reduction from one point of time to another in terms of the first power of the time. For this purpose we may make use of the form of development suggested by Struve.

Let $\frac{df^1}{dt}$ $\frac{df^2}{dt}$ $\frac{df^3}{dt}$ &c. represent the variations for one miuute at equal intervals of time, e. g. 16 minutes. We shall then have:

$$F=F_0+\frac{1}{90}\left(\frac{7\,df^1}{dt}+\frac{32\,df^2}{dt}+\frac{12\,df^3}{dt}+\frac{32\,df^4}{dt}+\frac{7\,df^5}{dt}\right)t$$

The successive values of $\frac{df^1}{dt}$ $\frac{df^2}{dt}$ &c. can be found by taking the difference between the computed values of $\frac{df}{dt}$ for the times $\overset{-1/4}{T}$ and $\overset{+1/4}{T}$.

Since considerable time must be allowed for the requisite adjustment after the standards have been removed to the comparator, we must rely upon the value of F computed from the equation at the instant when the standard was removed to the comparing room, but we cannot, unfortunately, make use of the observed data until after the exposure has been continued for at least three hours, since the thermometer never indicates the real temperature of the bar of metal while changes of temperature are in progress. Hence, if the latter formula is employed we must make the equal intervals large enough to reach a point of time near the end of the series, but the development will fail if the entire interval is too great.

In order to illustrate the application of this formula, we make use of the computed values of F derived from equation 12 of exhibit M. The interval chosen is 8 minutes.

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Time h m	F	$\frac{df}{dt}$
9 41.5	+ 10.623 div.	
9 42.0	14.106	+6.967 div.
9 42.5	17.590	
9 49.4	+ 61.056	
9 40.0	63.836	+5.559
9 50.5	66.615	
9 57.5	+101.103	
9 58.0	103.286	+4.371
9 58.5	105.474	
10 5.5	+132.724	
10 6.0	134.452	+3.457
10 6.5	136.181	
10 13.5	+157.721	
10 14.0	159.086	+2.733
10 14.5	160.432	

We have, therefore, for the reduction from $9^h\ 42^m$ to $10^h\ 14^m$

$$F=\frac{32}{90}\left[7\times6.967+32\times5.559+12\times4.371+32\times3.457+7\times2.733\right]=$$

145.4 div. The variation in length between these limits of time, derived from the differential equation, is equal to [159.1—14.1]=145.0 div.

It will not escape attention that the equations given on page 69 represent only the given series of observations from which they were derived. For example, the equation derived from the comparisons of experiment 1 will hold for any point of time during an interval of five hours and for any temperature between 34° and 64°, if we start at 34°, but it will not necessarily hold if we start, for example, at 35° and go to 65°. Within the short time since this line of investigation was undertaken no satisfactory analytical expression has been found from which the length of a given

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standard at any assumed time and for any given temperature can be determined.

(17) *Under variations of temperature not exceeding two degrees per hour the maximum error possible arising from the failure of the bar of metal to follow the indication of the thermometer placed upon its upper surface, is not far from 4 mikrons in the determination of the relative lengths of the bronze and the steel bars and not far from 10 mikrons in the case of the bronze and the glass bars.*

If the thermometer is placed at the center of the mass of the bar, an error of nearly the same amount but with the opposite sign, will be introduced. This law seems to hold true in the case of either a slow or a rapid change of temperature, but further observations are needed to determine the law in passing, at the stationary point, from a rising to a falling temperature and *vice versa.*

(18) *The limit of the maximum error, stated under the previous division, varies with the mass of the material of which the thermometers are constructed.*

The diameter of the stem and the mass of the metal backing which sometimes forms the support of the thermometer are the two elemnnts which have the greatest influence.

(19) *Any thermometer employed to measure the temperature of the standard bars compared, must have its errors determined under the same conditions as those under which the standard thermometer is exposed.*

Hence, the errors of secondary thermometers must be determined for a horizontal position and for contact with the same piece of metal as the primary standard.

The Yale Centigrade thermometer Y_{62} has been adopted as the primary standard in this investigation, and whenever any other thermometer has been employed its errors have been determined by comparison with Y_{62}.

(20) *Every thermometer employed to measure the temperature has its individual idiosyncrasies which must be studied in detail independently of its normal relations to the primary standard.*

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These peculiarities depend, in a general way, upon the form and the mass of the thermometers. As an illustration of the necessity of this precaution, it may be mentioned that certain thermometers of Series I invariably give wrong indications under certain conditions, the errors of the same point determined at equilibrinm being the standard of comparison. So also, the relations determined from day and from night observations are for some thermometers quite different from the relations which exist when the metal backing is removed and only the glass tube is used.

We have a test of the accuracy of the results obtained under exhibits K and L, in their accordance with the results obtained in previous years. In the following table are given the values of R_2—R_3 and of R_2—G_a, both for the yard adn the meter. In the case of the yard these values have been corrected by the addition of 9.8 div. for R_2—R_3 and of 1.1 div. for R_2—G_a. In the case of the meter the constants have not been taken out.

For the Yard.

Date.	At 62.0° R_2—R_3	No. Obs.	At 62.0° R_2—G_a	No. Obs.
Sept. 25,	—2.30 div.	2	—2.80 div.	2
26,	+5.20	2	+7.10	2
27,	—2.50	2	+7.95	2
28,	—0.89	7	—2.90	7
29,	—0,70	15	+3.94	17
30,	—2.59	7	+1.17	7
Oct. 2,	+0.50	3	—2.03	3
3,	—0.12	7	+3.14	7
4,	—1.70	7	+2.91	7
5,	—1.76	8	—1.01	8
6,	—0.17	3	+5.83	3
Means	—0.97		+1.18	

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For the Meter.

Date.	At 61.°0 R_2—R_3	No. Obs.	At 62.°0 R_2—G	No. Obs.
Oct. 10,	—2.05 div.	2	+15.45 div.	2
11,	—3.69	7	+11.34	7
12,	—3.20	7	+ 8.63	7
13,	—3.62	6	+16.05	6
14,	—3.20	2	+19.05	2
16,	—4.20	5	+18.16	5
17,	—4.40	2	+23.10	2
18,	—10.3	1	+ 8.1	1
25,	—6.60	4	——	–
27,	—6.76	14	——	–
Means	—4.84		+14.04	

For the yard, therefore, the deviation from the result given by the former series does not in either case much exceed half a mikron. For the meter we have, after the reduction for the horizontal curvature is applied, the following values for the corrections to be applied to R_3 and to G_1:

Correction to R_3 from the present series = $+3.5\mu$.

Correction to R_3 from the observations of 1884-5 = $+2.8\mu$.

Correction to G_1 from the present series = $+24.8\mu$.

Correction to G_1 from the observations of 1884-5 = $+21.4\mu$

It is probable that the first published value of the correction to G_1, viz. $+21.4\mu$ is erroneous, notwithstanding the close agreement between the early and the later values for the yard, since the necessity of making the comparisons only at times about three hours subsequent to an apparent equilibrium of temperature between the standards and the surrounding air, was not at that time recognized.

In conclusion, attention is again called to the fact that this investigation relates only to the constancy of the relative coefficients of expansion between these particular bars

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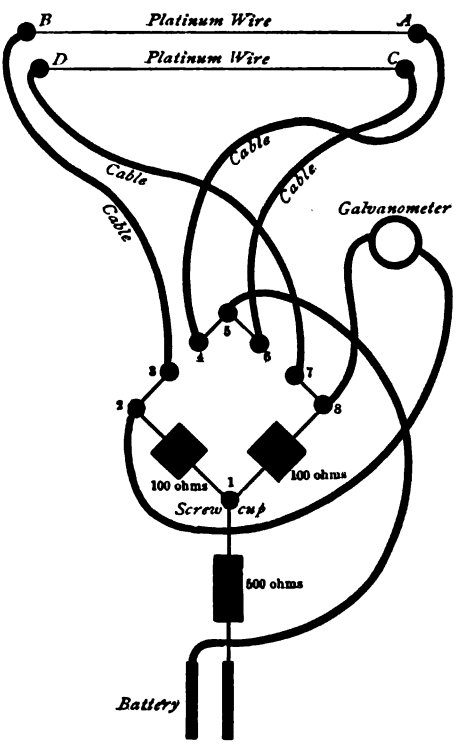
of metal and only between the limits of the temperatures at which actual observations have been made.

It may well be doubted whether the attempt to limit the comparisons to the imposed condition of molecular rest has been successful in this series of experiments, but the writer is confident that a near approximation to the exact relation between the standards R_2 and R_3 has been obtained. It is certainly true that under the conditions imposed a constant value of R_2—R_3 at 62.0° Fahr. will be obtained from observations at any temperature between 0° and 95°.

Every indication points to the conclusion that further advance in metrological investigation must be along the line indicated in this paper. The writer is now engaged in experiments with a modified form of Langley's bolometer which gives promise of most interesting and valuable results. The full account of the method will be reserved for a subsequent paper, but the general outline of the method will be seen from the following figure and the accompanying description.



The wire AB is insulated at the center of the standard whose temperature is

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desired. CD is another wire stretched along and very near the upper surface, also insulated from the bar. The galvanometer will then indicate the difference between the temperature of the upper and the lower wires, and with a close approximation between that of the surface and that of the central part of the bar. Eventually, probably a third wire will be stretched along the lower face of the standard which will form a pair with a second line through the central part, the connections being made in such a manner as to allow the necessary changes in connections to be made at the galvanometer.

The practical outcome of this investigation may be stated as follows:

(*a*) The normal dimensions of matter are obtained from direct observations only when it is in a state of approximate molecular rest.

(*b*) In the comparison of standards of length the best results will be obtained by delaying the observations until the standard bars of metal have remained for three and one-half hours at a temperature which at no time during this interval has changed more than two or three tenths of a degree.

(*c*) In the comparison of thermometers the best results will be obtained by delaying the observations until they have remained at a constant temperature for twenty minutes under the condition of open air exposure, while for the condition of contact with the surface of metals the indication will be largely controlled by the temperature of the metal.

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SERIES I.—July 8, 1887. EXHIBIT (A).

Temperature of mercury and of water after removal from melting ice to a nearly constant air temperature of 82° Fahr.

Number of vessel.	Material.	Hight.	Diameter.	Hight of liquid.	No. of cubic Centimeters of liquid.	Liquid.
1	Glass.	13 in.	2 in.	2.4 in.	100	Mercury and Water.
2	"	6.1 "	2¼ "	4.8 "	300	Mercury.
3	"	10.0 "	10 "	9.3 "	100	Water.
4	Porcelain.	....	....	....	100	Mercury.

Reading of Thermometers. Exposure at $10^h\ 5^m$. No. 2 removed in advance of Nos. 1, 3, and 5.

Time.	Stationary Thermometer.	No. 1. Mercury.		No. 1. Water.		No. 2.		No. 3.		No. 4.
		Bottom.	Top.	Bottom.	Top.	Bottom.	Top.	Bottom.	Top.	
h m	°	°		°		°		°		°
10 10	81.2	39.0	....	37.5	....	43.6	....	43.6	....	51.0
10 15	81.0	45.8	....	44.0	....	49.6	....	50.0	....	58.8
10 20	80.8	49.9	....	48.8	....	53.6	....	54.1	....	62.4
10 25	81.0	52.8	....	52.0	....	56.1	....	57.2	....	65.0
10 30	81.2	55.9	55.5	55.6	56.2	59.6	60.8	60.1	64.0	67.7
10 35	81.2	59.0	59.1	59.1	59.7	62.2	63.7	63.8	67.0	70.9
10 45	81.3	63.0	62.9	63.0	63.8	64.9	66.9	67.0	69.8	72.0
10 55	81.5	66.0	65.9	66.0	66.2	68.1	69.0	69.6	71.6	73.7
11 05	81.8	68.5	68.7	68.9	69.0	70.8	71.4	72.0	74.0	74.9
11 15	81.7	70.2	70.4	70.7	70.8	72.4	73.1	73.8	75.5	75.8
11 25	81.8	72.4	72.6	72.6	72.9	74.2	74.9	75.9	76.4	77.1

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Reading of Thermometers.—*Continued.*

Time.	Stationary Thermometer.	No. 1. Mercury.		No. 1. Water.		No. 2.		No. 3.		No, 4.
		Bottom.	Top.	Bottom.	Top.	Bottom.	Top,	Bottom.	Top.	
h m										
11 35	81.8	73.4	73.5	73.5	73.7	75.1	75.7	76.9	77.8	76.9
11 50	81.9	75.1	75.2	75.3	75.2	76.7	77.0	78.1	79.0	77.1
12 05	82.0	76.2	76.3	76.3	76.3	77.4	77.9	78.8	79.5	77.4
12 30	82.1	78.2	78.1	78.3	78.5	78.0	79.2	80.0	80.9	77.8
2 45	82.1	80.6	80.7	80.7	80.7	80.8	80.8	81.7	82.0	78.6
3 00	82.2	81.0	81.0	81.0	81.0	80.9	80.8	81.7	82.0	78.7
3 10	Lowered position of every vessel 10 inches.									
3 45	82.2	81.5	81.4	81.5	81.5	81.2	81.2	81.7	82.0	79.0
3 50	Raised every vessel 6 inches above first position.									
4 00	82.4	81.8	81.8	81.8	81.8	81.6	81.6	82.1	82.1	79.2
4 15	82.4	82.0	81.9	81.9	81.9	81.7	81.6	82.1	82.1	79.5
4 30	82.4	82.0	82.0	81.9	81.9	81.7	81.7	82.1	82.1	79.7
4 35	Lowered every vessel to the first position.									
4 40	82.5	82.0	82.0	....	....	81.7	81.7	82.2	82.3	80.0
5 15	82.5	82.0	82.0	....	....	81.8	81.8	82.3	82.3	80.1
5 30	82.4	82.1	82.0	....	....	81.8	81.8	82.3	82.3	80.1
7 00	82.2	82.0	82.0	....	....	81.8	81.8	82.2	82.2	80.5

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Series II.—July 14th and 15th.

Temperatures of mercury and of water exposed in tin vessels.

Removed from melting ice at $8^h\ 45^m$.

			Mercury					Water				
No. inches in diameter,			2	2	2	3	6	2	2	2	3	6
No. cubic centimeters,			50	100	150	100	100	50	100	150	100	100
Time.	h	m	Air Temperatures.									
July 14.—	8	55	65.73	56.0	55.0	55.1	54.6	53.4	42.8	39.7	37.1	43.0	46.8
	9	05	65.80	61.8	60.4	60.4	60.1	59.2	50.0	46.0	42.9	50.0	53.2
	9	20	65.80	63.2	62.6	62.6	62.2	62.0	54.2	50.7	49.8	54.3	57.0
	9	30	65.95	64.1	63.3	63.3	63.3	63.3	58.2	54.6	52.7	57.2	59.7
	10	10	66.03	65.0	64.7	64.8	65.1	65.0	61.4	59.4	59.5	62.0	63.0
	10	30	66.10	65.1	64.7	64.8	65.0	64.9	63.1	62.1	61.5	63.2	64.3
	10	40	66.27	64.9	64.8	64.8	65.0	65.0	63.9	62.6	62.0	63.5	64.1
	12	00	65.93	65.0	65.0	65.0	65.1	65.2	64.9	64.6	64.5	65.0	65.3
	12	20	66.15	65.1	65.1	65.0	65.2	65.3	64.9	64.8	64.7	65.1	65.2
	1	30	66.06	65.2	65.3	65.2	65.7	65.6	65.0	65.0	64.9	65.2	65.4
	3	30	66.31	65.2	64.9	64.8	65.2	65.7	66.0	65.8	65.7	66.0	66.0
	5	47	66.69	66.2	66.0	66.0	66.0	66.0	65.8	65.2	65.2	65.7	66.0
	6	40	66.78	66.2	66.1	66.2	66.2	66.1	65.9	65.3	65.2	66.0	66.1

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SERIES II.—July 14th and 15th.—*Continued.*

			Mercury					Water				
No. inches in diameter,			2	2	2	3	6	2	2	2	3	6
No. cubic centimeters,			50	100	150	100	100	50	100	150	100	100
Time.	h	m	Air Temperatures.									
July 15th.	5	00	65.90	65.°9	65.°9	65.°8	65.°7	66.°0	65.°9	65.°9	65.°9	65.°1	65.°3
	5	10	Removed from comparing room to melting ice.									
	5	13	.....	55.0	53.0	52.0	50.7	48.1	56.1	52.1	51.8	49.9	53.9
	5	18	.....	45.5	43.3	42.9	42.9	41.0	50.5	46.6	46.8	45.8	48.2
	5	23	.....	41.1	40.0	39.3	39.1	40.2	44.3	42.1	42.8	41.2	45.2
	5	28	.....	38.8	37.9	37.2	37.1	38.7	42.0	40.0	40.0	38.7	42.6
	5	33	.....	37.5	36.8	36.2	36.1	38.1	39.4	37.8	37.5	36.0	42.0
	5	38	.....	36.0	36.1	35.8	35.6	38.2	38.1	37.4	35.8	35.8	39.8
	5	43	.....	36.0	35.5	34.9	35.2	37.8	37.0	36.4	35.1	35.8	39.1
	6	20	.....	35.6	35.3	35.0	34.7	37.2	34.2	34.2	34.8	34.8	37.3
	6	25	.....	35.5	35.0	34.9	34.5	37.0	34.0	34.4	34.8	34.6	37.4
	7	10	66.0	35.8	36.1	35.2	34.7	37.0	33.2	34.2	33.9	34.1	37.2

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EXHIBIT (B).

Observed relations between the bronze and steel standards of length.

METERS.

Date of Comparisons.	Comparison with	Equations between R_s and A_0.	Weights.
1884, Feb. 5 to Mar. 11.	R_2 (meter)	$R_s+4.0\mu=A_0$	1
" " "	R_2 (yard)	$R_s+4.1\mu=A_0$	1
" " "	R_1 (meter)	$R_s+2.4\mu=A_0$	1
" " "	R_1 (yard)	$R_s+2.7\mu=A_0$	1
" Nov. 16 to Dec. 21.	R_2	$R_s-1.2\mu=A_0$	½
" Dec. 21 to Dec. 31.	R_2	$R_s+2.2\mu=A_0$	2
1885. Jan. 2 to Feb. 15.	R_2	$R_s+1.9\mu=A_0$	3

Adopt. $R_s+2.4\mu=A_0$.

YARDS.

1884. Feb. 5 to Mar. 11.	R_2 (yard)	$R_s+1.0\mu=Y$	1
" " "	R_2 (meter)	$R_s-1.4\mu=Y$	1
" " "	R_1 (yard)	$R_s-0.6\mu=Y$	1
" " "	R_1 (meter)	$R_s-0.8\mu=Y$	1
1885, Jan. 12 to Feb. 16.	R_2	$R_s-0.5\mu=Y$	1

Adopt. $A_s-0.5\mu=Y$.

METERS.

Date of Comparisons.	Comparison with	Equations between A_4 and A_0.	Weights.
1884, Feb. 5 to Mar. 11.	R_2	$A_4+4.0\mu=A_0$	2
" Mar. 12 to Mar. 21.	R_2	$A_4+1.3\mu=A_0$	2
" Feb. 5 to Mar. 5.	R_1 (meter)	$A_4-0.4\mu=A_0$	1
" " "	R_1 (yard)	$A_4+0.0\mu=A_0$	1
1885, Jan. 10 to Feb. 15.	R_2	$A_4-0.9\mu=A_0$	3

Adopt. $A_4+0.2\mu=A_0$.

YARDS.

1884, Feb. 5 to Mar. 11.	R_2	$A_4+1.8\mu=Y$	2
" Mar. 12 to Mar. 27.	R_2	$A_4+1.3\mu=Y$	2
" Feb. 5 to Mar. 5.	R_1 (yard)	$A_4+1.4\mu=Y$	1
" " "	R_1 (meter)	$A_4+0.2\mu=Y$	1
1885, Jan. 12 to Feb. 24.	R_2	$A_4+1.8\mu=Y$	3

Adopt. $A_4+1.1\mu=Y$.

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EXHIBIT C.

Relative lengths of steel and silver strips corresponding to variations of temperature between the limits +18° C. and +39° C.

Tension of strips=7 lbs.

I=increasing temperature.

D=falling temperature.

S=stationary temperature.

Δ=excess of micrometer reading of silver over steel.

1°=15.0 divisions of micrometer.

Date.	Y_{68}	28°.00—Y_{68}	Δ	Δ reduced to 28°.00	Number of Comparisons.	Direction of Temperature.
1887.	°	°				
July 4.	29.88	— 1.88	+ 89.6 div.	+61.4 div.	5	D
" 4.	29.48	— 1.48	+ 82.7	+60.5	2	I
" 5.	27.92	+ 0.08	+ 54.9	+53.7	4	S
" 5.	28.92	— 0.92	+ 47.2	+61.0	2	S
" 6.	23.97	+ 4.03	+ 0.0	+60.4	3	S
" 8.	28.21	— 0.21	+ 51.0	+54.2	2	I
" 8.	28.08	— 0.08	+ 49.0	+50.2	3	D
" 9.	27.64	+ 0.36	+ 62.0	+67.4	8	I
" 10.	18.60	+ 9.40	— 69.3	+71.7	1	D
" 11.	18.13	+ 9.87	— 72.9	+75.1	3	I
" 12.	25.25	+ 2.75	+ 24.5	+65.7	4	D
" 12.	29.00	— 1.00	+ 59.4	+44.4	2	D
" 13.	24.65	+ 3.35	+ 1.3	+51.5	6	D
" 13.	29.10	+ 1.10	+ 89.3	+72.8	3	I
" 14.	20.50	+ 7.50	— 61.8	+50.7	2	D
" 14.	38.57	—10.57	+215.7	+57.2	3	D
" 14.	37.75	— 9.75	+210.7	+64.5	7	I
" 14.	37.88	— 9.88	+211.2	+63.0	8	D
" 15.	21.39	+ 6.61	— 31.7	+67.4	7	I
" 16.	20.34	+ 7.66	— 47.4	+67.5	1	I
" 16.	20.27	+ 7.73	— 60.4	+55.5	8	D
" 18.	19.17	+ 8.83	— 68.4	+64.0	1	I
" 18.	19.73	+ 8.27	— 68.2	+55.8	3	S

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EXHIBIT C.—*Continued.*

Equations of condition from groups of five observations each. All the observations are assumed to have equal weights.

	$(28.^{\circ}00-T)\Delta b$	a	Normal Equations.
+64.4 div.	$=a+8.81\Delta b$	+63.3	$+303.4=+\ 5a+\ 5.71\Delta b$
59.5	$=a+5.43$	+58.9	$+365.8=+5.71a+172.5\Delta b$
58.3	$=a+0.58$	+58.2	
59.7	$=a-1.12$	+59.8	$\Delta b=+\ 0.12$
61.5	$=a-7.99$	+62.5	$a=+60.6$

The coefficient between steel and the silver is therefore 15.12 div.

Hence 1 div. $=0.^{\circ}066$ C.

Residuals in divisions.	Residuals in terms of 1 degree C.
+2.6 div.	$+0.^{\circ}17$
—1.6	—0.11
—2.3	—0.15
—0.7	—0.05
+2.0	+0.13

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EXHIBIT D.

Comparison of thermometers Y_{62} and G_2, which rest upon the surface of a large mass of iron with G_1, which is supported in air at nearly the same height as Y_{62} and G_2.

October 1, 1886.

Time.	h m 8 15	h m 8 30	h m 8 45	h m 9 00	h m 9 15	h m 9 35	h m 10 00	h m 10 30	h m 10 50	h m 11 00	h m 11 15
Y_{62}	16°.19	15°.45	16°.73	17°.24	18°.66	20°.88	23°.25	26°.00	27°.19	27°.71	28°.76
G_1	16.30	18.18	21.32	24.18	26.40	29.05	31.12	33.60	34.24	34.24	34.22
G_2	15.88	16.32	17.38	18.63	20.11	22.88	25.10	27.82	28.87	29.38	29.98
$Y_{62}-G_1$	—1.11	—2.73	—4.59	—6.94	—7.74	—8.17	—7.87	—7.60	—7.05	—6.53	—5.46
$Y_{62}-G_2$	—0.69	—0.87	—0.65	—1.39	—1.45	—2.00	—1.85	—1.82	—1.68	—1.67	—1.22

Time.	h m 11 30	h m 12 0	h m 12 30	o m 1 50	h m 2 15	h m 2 50	h m 3 0	h m 3 30	h m 3 45	h m 4 15	
Y_{62}	29°.08	30°.17	30°.62	29°.32	28°.97	28°.72	28°.76	28°.76	28°.98	29°.36	°
G_1	33.76	34.58	31.59	29.72	29.68	29.52	29.93	30.79	31.61	32.11	
G_2	30.44	31.48	31.42	29.85	29.57	29.32	29.46	29.50	29.82	30.12	
$Y_{62}-G_1$	—4.68	—4.41	—0.97	—0.40	—0.71	—0.80	—1.17	—2.03	—2.63	—2.75	
$Y_{62}-G_2$	—1.36	—1.31	—0.80	—0.53	—0.60	—0.60	—0.70	—0.74	—0.84	—0.76	

Explanatory Notes.

h m 8 0	Lighted gas under water tank.
10 41	Turned down gas as low as possible.
11 44	Turned on full flow of gas.
11 57	Turned gas down sufficient to hold the temperature at 30° C.
12 21	Opened door of comparing room.
2 11	Turned on full flow of gas.

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EXHIBIT E.

Comparisons of yards R_2 R_3 G_1 and G_5, expressed in divisions of the micrometer of which one division = 0.567μ.

Observed data.

From —2° to +10°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1886, Dec. 30.	—2.44°	—403.8 div.	—528.6 div.	—516.7 div.
1887, Jan. 5.	—1.06	—410.8	—540.3	—516.7
" " 5.	—0.66	—401.0	—540.3	—526.6
" " 20.	—0.27	—407.4	—529.4	—514.0
1886, Dec. 31.	+1.12	—389.1	—503.7	—485.6
1887, Jan. 30.	+2.31	—364.1	—515.8	—497.0
1886, Dec. 31.	+4.09	—369.0	—485.9	—474.9
1887, Jan. 11.	+4.09	—369.8	—473.9	—454.1
" " 17.	+4.79	—360.3	—465.0	—444.5
" " 10.	+5.76	—367.8	—487.7	—472.9
" " 14.	+5.96	—348.5	—469.8	—451.4
" " 9.	+6.46	—357.8	—472.3	—452.3
" " 11.	+6 56	—352.3	—488 0	—469.9
" " 14.	+6.86	—351.2	—469.1	—449.0
" " 28.	+6.91	—349.0	—461.0	—442.3
" " 14.	+7.06	—365.9	—486.7	—468.3
" " 14.	+7.84	—346.9	—457.7	—437.7
" " 14.	+8.15	—340.9	—447.4	—424.2
" Jan. 3.	+8.30	—333.8	—439.5	—420 0

From +10° to +20°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, Jan. 3.	+11.26°	—341.2	—457.7	—437.5
1886, Dec. 28.	11.86	—326.3	—414.0	—395.7
1887, Jan. 3.	13.15	—325.7	—449.0	—430.5
1886, Dec. 27.	15 12	—294.4	—400.1	—381.9
" " 27.	15.91	—284.4	—392.7	—376.9
1887, Apr. 1.	15.91	—289.3	—371.9	—353.3
" Mar. 27.	17.19	—288.9	—376.4	—359.6
" " 30.	18.64	—278.9	—365.5	—334.5
" " 30.	20.32	—270.1	—350.9	—333.2

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EXHIBIT E.—*Continued.*

From +20° to +30°.

Date.	T.	R_2-R_6	R_2-G_1	R_2-G_5
1887, Jan. 16.	+20°.95	—273.0 div.	—353.3 div.	—333.7 div.
" " 16.	21.45	—265.2	—348.5	—329.2
1886, Dec. 26.	21.98	—236.4	—314.5	—296.4
1887, Apr. 8.	22.04	—255.0	—338.2	—317.6
1886, Dec. 26.	22.66	—240.1	—308.1	—282.9
1887, Jan. 16.	22.73	—262.7	—357.2	—340.5
1886, Dec. 22.	23.33	—253.0	—331.0	—312.0
1887, Mar. 24.	23.53	—254.9	—344.5	—323.4
" Jan. 12.	23.79	—248.1	—312.1	—301.9
" " 16.	24.33	—251.7	—336.6	—322.9
" Mar. 31.	24.93	—232.5	—339.8	—320.3
" Jan. 26.	25.09	—236.0	—315.4	—295.7
1886, Dec. 22.	25.83	—235.8	—312.5	—291.0
1887, Mar. 24.	26.22	—248.8	—337.6	—320.2
" Jan. 5.	27.08	—234.4	—318.4	—304.0
" " 18.	28.36	—212.5	—295.5	—275.5
1886, Dec. 19.	28.51	—224.9	—290.0	—270.0
1887, Apr. 18.	29.49	—208.6	—270.1	—253.2
" " 18.	29.49	—209.8	—279.7	—263.1
1886, Dec. 20.	29.66	—212.0	—281.0	—261.0
1887, Apr. 18.	29.88	—205.0	—264.4	—250.9
1886, Dec. 26.	29.94	—203.6	—256.5	—243.9
1887, Jan. 23.	29.98	—213.7	—276.6	—259.9
1886, Dec. 26.	30.05	—199.2	—260.0	—245.4

From +30° to 40°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, Mar. 29.	30°.29	—223.1 div.	—304.0 div.	—282.7 div.
" Apr. 6.	30.37	—193.7	—247.9	—227.7
" " 13.	31.92	—201.9	—254.0	—235.7
" Jan. 24.	32.13	—202.9	—261.1	—245.6
1886, Dec. 18.	32.50	—198.0	—257.1	—238.2
1887, Jan. 13.	32.92	—202.6	—291.5	—273.7
" Mar 30.	33.01	—189.6	—243.7	—216.5
" Jan. 2.	33.06	—189.3	—246.1	—223.0
1886, Dec. 18.	33.12	—196.7	—252.3	—233.3
1887, Jan. 2.	33.13	—184.2	—258.9	—239.6
" " 2.	33.88	—188.8	—253.8	—236.8
" Apr. 14.	34.08	—184.2	—245.4	—224.1

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EXHIBIT E.—*Continued.*

From +30° to 40°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, Apr. 21.	34.61°	—170.6 div.	—226.0 div.	—210.6 div.
" " 21.	34.69	—173.0	—228.6	—209.6
" " 12.	35 00	—181.6	—258.5	—234.5
" " 21.	35.00	—174.6	—231.7	—212.8
" Jan. 25.	35.22	—173.6	—220.6	—201.7
" Apr. 7.	35.58	—172.8	—223.2	—254.4
" " 7.	35.72	—170.1	—209.7	—189.2
" Mar. 20.	36.36	—168.6	—219.6	—201.9
" Apr. 7,	36.61	—170.6	—234.0	—214.5
" " 27.	36.76	—163.0	—219.4	—200.8
" Mar. 25.	36.86	—158.5	—227.6	—208.3
" Apr. 7.	36.96	—157.5	—211.8	—192.1
" Jan. 7.	37.21	—158.6	—214.4	—193.5
1886, Dec. 21.	37.90	—158.8	—213.3	—194.4
" " 21.	38.08	—152 1	—211.7	—193.2
1887, Jan. 26.	38.28	—155.3	—202.3	—184.9
" " 26.	38.35	—155.1	—205.9	—188.5
" Apr. 7.	38.37	—172.1	—208.8	—178.8
" Jan. 21.	38.52	—151.6	—221.7	—203.4
" Apr. 4.	38.84	—165.0	—220.0	—203.1
" Mar. 22.	38.99	—149.5	—219.5	—198.3
" Apr. 8.	39.59	—162.1	—220.9	—201.8
" Jan. 26.	39.84	—146.0	—191.1	—172.9

From +40° to +50°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_3
" Mar 19.	39.95°	—145.9	—205.6	—178.1
1886, Dec. 23.	40.88	—136.6	—195.0	—181.0
" Dec. 23.	41.19	—129.8	—197.6	—179.0
1887, Apr. 5.	41.25	—147.9	—196.1	—178.5
" Apr. 5.	41.65	—141.1	—197.5	—177.7
" Mar. 17,	41.87	—143.0	—181.5	—162.8
" Apr. 5.	41 92	— 144.6	—197.2	—180.6
1886, Dec. 24.	42.02	—112.5	—181.0	—163.0
1887, Mar. 17.	42.37	—128.6	—186.6	—168.5
" Apr. 7.	42.44	—141.2	—191.0	—171.2
" Apr. 11.	42.59	—118.2	—183.6	—166.2
1886, Dec. 24.	42.66	—115.4	—153.4	—135.9
1887, Mar. 17.	43.42	—130.4	—180.7	—160.4

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EXHIBIT E.—*Continued.*

From +40° to +50°.

	Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
		°			
1887,	Mar. 21.	43.42	—122.1 div.	—158·9 div.	—140.3 div.
		45.72	—133.0	—158.7	—138.7
"	Apr. 19.	46.02	—109.0	—139.2	—110.8
1887,	" 10.	46.82	—100.4	—142.3	—123.1
"	" 17.	47.07	—115.3	—148.3	—131.7
"	Apr. 20.	47.82	—111.5	—151.4	—133.5
"	Mar. 20.	47.82	— 96.7	—126.4	—107.7
"	Apr. 17.	48.07	—106.3	—143.6	—125.1
"	Mar. 20.	48.12	— 95.2	—134.3	—119.0
"	" 20.	48.12	— 94.9	—130.7	—109.7
"	May 13.	48.37	— 90.0	—123.9	—106.9
"	" 13.	48.52	— 94.4	—122.5	—106.7
"	Mar. 20.	48.62	— 95.6	—134.4	—116.1
"	Apr. 11.	48.82	— 90.1	—140.8	—120.9
"	Mar. 17.	48.82	— 83.5	—138.7	—123.4
"	May 16.	49.25	— 83.6	—116.6	—100.3
"	Apr. 29.	49.87	— 87.8	—127.7	—102.5

From +50° to +60°.

	Date.	T.	R_2-R_3	R_2-G_1	R_1-G_5
		°			
1887,	Apr. 25.	49.92	— 82.4	—110.4	— 91.1
"	May 16.	49.92	— 84.6	—121.2	—103.0
"	Apr. 25.	50.02	— 82.5	—112.7	— 93.0
"	May 12.	50.77	— 81.8	—105.7	— 87.3
"	" 12.	50.77	— 81.7	—109.6	— 87.8
"	Apr. 17.	51.12	— 87.9	—117.3	— 99.1
"	May 12.	51.27	— 74.8	—111.0	— 93.4
"	Apr. 29.	51.67	— 84.0	—124.6	—106.5
"	" 29.	51.91	— 77.5	—107.0	— 88.2
"	May 12.	57.92	— 80.6	—114.9	— 96.8
"	" 1.	51.97	— 81.6	—104.8	— 77.0
"	" 1.	51.22	— 74.7	—105.4	— 84.2
"	Apr. 27.	52.71	— 60.4	— 87.4	— 66.0
"	" 29.	52.81	— 70.0	—113.5	— 93.9
"	" 29.	52.81	— 64.9	—100.2	— 82.4
"	" 29.	52.91	— 60.1	—101.8	— 84.6
"	" 27.	52.91	— 62.6	— 86.0	— 66.0
"	" 29.	53.01	— 71.7	—110.7	— 91.9
"	" 29.	53.01	— 69.0	—105.6	— 87.5

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EXHIBIT E.—*Continued.*

From +50° to +60°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, Apr. 29.	53°.16	— 65.1 div.	—108.3 div.	— 89.7 div.
" " 11.	53.49	— 67.2	— 86.3	— 70.4
" " 24.	54 76	— 49.6	— 79.6	— 59.9
" " 24.	54.85	— 50.8	— 77.7	— 59.1
" " 24.	54.87	— 48.5	— 71.7	— 54.9
" " 24.	54.90	— 57.0	— 80.8	— 64.1
" " 24.	54.93	— 52.3	— 80.8	— 59.7
" " 24.	54.95	— 51.2	— 81.3	— 66.3
" " 24.	54.95	— 54.5	— 85.3	— 65.8
" " 24.	55.00	— 46.5	— 71.1	— 52.5
" " 27.	55.00	— 48.7	— 76.1	— 54.1
" " 27.	55.05	— 46.2	— 66.5	— 46.0
" " 27.	55.05	— 50.4	— 70.5	— 52.2
" " 24.	55.10	— 48.4	— 66.6	— 48.5
" May 30.	55.81	— 44.3	— 69.2	— 49.5
" " 30.	55.87	— 44.3	— 71.4	— 49.9
" " 30.	55.92	— 43.3	— 69.8	— 49.8
" " 19.	56.44	— 39.1	— 51.9	— 36.5
" " 30.	56.57	— 42.6	— 73.3	— 56.2
" " 30.	56.81	— 40.2	— 71.7	— 52.3
" " 15.	57.05	— 44.0	— 67.5	— 50.2
" " 8.	57.35	— 40.5	— 56.7	— 36.9
" " 30.	57.51	— 35.0	— 57.8	— 39.7
" " 15.	57.64	— 38.2	— 61.3	— 43.2
" " 1.	57.99	— 32.5	— 53.3	— 34.5
" " 1.	58.04	— 28.1	— 44.2	— 26.4
" " 19.	88.44	— 31.7	— 58.3	— 36.6
" " 15.	58.62	— 27.9	— 35.9	— 16.5
" " 15.	58.72	— 27.7	— 33.9	— 16.7
" " 23.	58.73	— 34.6	— 66.4	— 49.5
" " 19.	58.92	— 31.5	— 49.8	— 32.1

From +60° to +70°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, May 18.	60°.22	—21.9	—30.6	—10.6
" " 17.	60.27	—12.5	—26.5	— 7.0
" " 18.	60.32	—17.0	—34.7	—17.5
" " 17.	60.47	—18.4	—25.4	— 8.7
" " 18.	60.82	— 9.4	—18.1	+ 0.2
" " 4.	60.92	— 7.7	—18.5	+ 0.8

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EXHIBIT E.—*Continued.*

From +60° to +70°.

Date.			T.	R_2-R_3	R_2-G_1	R_2-G_5
			°			
1887,	May	4.	60.97	—11.8 div.	—20.4 div.	+ 1.2 div.
"	"	10.	62.01	— 7.3	—11.6	+ 9.3
"	"	10.	62.16	— 9.2	—11.4	+ 9.3
"	"	6.	62.16	+ 0.4	— 5.9	+14.0
"	"	10.	62.41	— 3.2	—14.8	+ 9.8
"	"	10.	62.46	— 4.1	— 8.4	+ 3.0
"	"	6.	62.61	— 4.2	—19.0	— 2.8
"	"	9.	62.16	— 5.5	—10.7	+ 5.3
"	"	9.	63.31	— 8.7	—10.0	+ 7.6
"	"	20.	63.31	+ 4.7	— 7.9	+10.2
"	"	9.	63.41	— 0.6	— 6.8	+14.0
"	"	9.	63.51	— 3.4	—16.5	+ 3.6
"	"	9.	63.66	— 2.6	—15.9	+ 4.6
"	"	6.	63.71	— 4.4	—17.7	+ 0.2
			64.05	— 7.3	—10.7	+ 2.6
"	"	4.	64.26	+ 1.1	— 1.7	+18.7
"	"	22.	66.71	+13.2	+12.2	+32.7
"	"	22.	67.01	+17.1	+14.9	+33.7
"	"	22.	67.41	+19.7	+18.9	+35.7
"	"	22.	67.41	+22.4	+14.6	+34.1
"	"	22.	67.51	+25 1	+18.5	+37.5
"	"	11.	67.61	+34.6	+26.2	+46.1
"	"	21.	67.61	+29.1	+32.8	+53.1
"	"	11.	67.76	+29.5	+33.4	+53.0
"	"	11.	68.51	+30.8	+24.5	+44.8
"	"	24.	68.81	+34.4	+30.6	+50.8
"	"	24.	68.81	+33.0	+28.9	+49.6
"	"	11.	68.81	+30.5	+38.5	+59.4
"	"	11.	68.81	+33.8	+42.0	+63.1
"	"	26.	68.86	+38.3	+35.9	+53.3
"	"	11.	68.86	+30.3	+35.5	+56.3
"	"	11.	68.91	+37.7	+44.9	+63.5
"	"	11.	68.96	+31.5	+40.3	+56.5
"	"	11.	69.11	+37.2	+38.8	+58.9

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EXHIBIT E.—*Continued.*

From +70° to +79°.

	Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
		°			
1887	May 15.	72.41	+ 56.6 div.	+ 58.8 div.	+ 70.7 div.
"	July 12.	72.57	+ 54.8	+ 53.4	+ 80.0
"	May 25.	72.62	+ 53.6	+ 68.2	+ 86.9
"	July 12.	72.68	+ 53.0	+ 53.0	+ 72.4
"	" 12.	72.81	+ 53.7	+ 52.8	+ 70.7
"	" 11.	73.11	+ 55.1	+ 63.8	+ 83.8
"	" 11.	73.13	+ 63.9	+ 74.9	+ 94.3
"	" 11.	73.15	+ 63.3	+ 75.1	+ 92.8
"	" 11.	73.28	+ 62.8	+ 72.0	+ 92.9
"	" 11.	73.32	+ 62.0	+ 75.1	+ 93.0
"	" 11.	73.40	+ 64.8	+ 75.8	+ 93.9
"	" 11.	73.40	+ 59.3	+ 74.7	+ 95.2
"	" 11.	73.45	+ 60.0	+ 74.9	+ 90.7
"	" 11.	73.49	+ 62.2	+ 73.8	+ 91.9
"	" 11	73.56	+ 61.7	+ 74.1	+ 92.0
"	" 10.	75.95	+ 79.0	+ 98.3	+117.8
"	" 10.	75.97	+ 78.3	+ 99.7	+118.3
"	" 10.	76.01	+ 80.1	+ 99.8	+118.3
"	" 10.	76.05	+ 80.2	+102.9	+120.7
"	" 10.	76.30	+ 82.5	+ 99.4	+120.2
"	" 10.	76.33	+ 75.4	+ 98.2	+117.2
"	" 10	76.57	+ 82.8	+101.1	+120.2
"	" 10.	76.41	+ 79.2	+101.8	+120.4
"	" 10.	76.77	+ 82.8	+103.5	+121.9
"	" 10.	76.77	+ 82.8	+107.3	+125.5
"	" 10.	76.79	+ 83.1	+105.4	+122.0
"	" 10.	76.92	+ 81.3	+108.0	+126.0
"	" 10.	76.92	+ 85.4	+107.5	+126.2
"	" 10.	77.15	+ 90.1	+112.2	+130.2
"	" 10.	77.17	+ 83.4	+111.3	+129.6
"	" 10.	77.23	+ 90.1	+112.7	+130.4
"	" 10.	77.38	+ 89.3	+110.8	+128.0
		77.64	+ 91.4	+111.6	+130.0
		77.83	+ 94.9	+112.6	+134.0
"	July 10.	78.13	+ 88.0	+109.7	+126.9

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EXHIBIT E.—*Continued.*

From +79° to +93°.

Date.			T.	R_2-R_3	R_2-G_1	R_1-G_5
1887,	July	8.	79°.66	+102.5 div.	+128.9 div.	+144.0 div
"	"	8.	79.68	+104.6	+130.1	+146.2
"	"	8.	79.72	+102.8	+130.0	+145.0
"	"	7.	79.74	+101.7	+127.3	+145.7
"	"	8.	79.86	+103.5	+130.2	+145.5
"	"	7.	79.86	+101.4	+121.4	+139.3
"	"	7.	80 02	+100.0	+126.7	+141.4
"	"	7.	80.05	+101.8	+127.1	+140.8
"	"	7.	80.26	+102.6	+123.7	+143.3
"	"	4.	82.07	+115.0	+148.5	+168.7
"	"	4.	82.09	+112.7	+141.5	+162.9
"	"	4.	82.12	+116.9	+147.9	+164.9
"	"	4.	82.24	+116.5	+148.4	+164.9
"	"	4.	82.66	+118.3	+147.2	+164.6
"	"	3.	84.11	+129.6	+163.7	+182.0
"	"	3.	84.11	+131.1	+165.1	+181.3
"	"	3.	84.16	+124.0	+159.1	+179.1
"	"	3.	84.19	+127.0	+161.1	+178.0
"	"	3.	84.34	+125.9	+156.6	+174.3
"	"	3.	84.49	+124.8	+159.0	+177.3
"	"	3.	84.57	+126.2	+158.5	+174.4
"	"	3.	84.66	+135.7	+164.5	+182.0
"	"	3.	84.76	+135.1	+155.9	+181.5
"	"	3.	84.78	+128.2	+164.5	+175.7
"	"	3.	84.80	+130.2	+162.5	+180.0
"	"	3.	84.80	+128.8	+158.4	+177.0
"	"	3.	84.86	+128.9	+160.2	+176.3
"	"	3.	84.87	+129.1	+162.3	+179.1
"	"	3.	84.90	+133.8	+162.2	+180.2
"	"	6.	86.29	+145.1	+185.9	+204.4
"	"	6.	86.34	+142.8	+185.0	+198.6
"	"	6.	86.61	+141.9	+185.5	+202.5
"	"	6.	86.62	+141.0	+177.5	+195.3
"	"	6.	86.87	+148.8	+190.4	+207.7
"	"	6.	87.07	+141.8	+181.6	+199.9
"	"	6.	87.16	+142.1	+179.1	+196.3
"	"	6.	87.16	+142.1	+180.5	+199.8
"	"	6.	87.81	+142.4	+181.5	+199.3
"	"	1.	88.46	+150.1	+193.0	+212.5
"	"	1.	88.56	+151.2	+194.0	+213.0
			88.68	+148.4	+197.9	+219.9

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EXHIBIT E.—*Continued.*

From +79° to +93°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, July 1.	88°.71	+151.7 div.	+204.6 div.	+223.4 div.
" " 1.	88.77	+146.3	+193.5	+212.0
" June 30.	88.76	+151.6	+191.2	+209.3
" " 30.	88.76	+154.9	+190.2	+210.5
" " 30.	88.84	+150.9	+191.5	+209.8
" " 30.	88.91	+154.7	+191.6	+209.2
" " 30.	88.91	+154.0	+193.6	+212.0
" July 5.	91.98	+178 8	+226.3	+244.7
" " 5.	92.21	+175.4	+228.3	+246.4
" " 5.	93.29	+182.3	+234.2	+252.9

From —20° to —14°.

Date.	T.	R_2-R_3	R_2-G_1	R_2-G_5
1887, Jan. 4.	—19°.80	—490.0	—645.8	—623.8
" " 9.	—13.80	—457.0	—614.0	—596.3
" " 19.	—11.85	—459.6	—614.0	—588.9

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EXHIBIT F.

Results in groups of ten degrees each.

T.	Δ	R_2-R_3	Δ	Coefficient for 1°.		R_2-G_1	Δ	Coefficient for 1°.		R_2-G_5	Δ	Coefficient for 1°.	
4.58°		−366.8 div.				−486.0 div.				−467.8 div.			4.
	11.05		6.87	6.22 div.	3.53μ		9.16	8.29 div.	4.70μ		93.0	8.42 div.	4.77μ
15.53		−298.1				−394.4				−374.8			
	10.52		6.52	6.20	3.52		8.62	8.19	4.64		84.2	8.00	4.54
26.05		−232.9				−308.2				−290.6			
	9.49		5.82	6.13	3.48		7.53	7.94	4.50		77.2	8.14	4.62
35.54		−174.7				−232.9				−213.4			
	9.64		5.99	6.21	3.52		7.34	7.61	4.31		72.8	7.55	4.28
45.18		−114.8				−159.5				−140.6			
	9.34		0.90	6.32	3.58		7.66	8.20	4.65		76.6	8.20	4.65
54.52		− 55.8				− 82.9				− 64.0			
	10.92		6.52	6.26	3.55		8.76	8.41	4.77		87.8	8.43	4.78
64.94		+ 9.4				+ 4.7				+ 23.8			
	10.17		6.39	6.22	3.53		8.49	8.27	4.69		84.3	8.21	4.65
75.21		+ 73.3				+ 89.6				+108.1			
	9.88		5.89	5.96	3.38		7.76	7.85	4.48		76.8	7.77	4.40
85.09		+132.2				+167.2				+184.9			
		Means		6.18 div.	3.51μ			8.09 div.	4.59μ			8 09 div.	4.59μ

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EXHIBIT G.

Results in groups of 10 observations each.

No.	T.	1°=6.17 div. R_2-R_3	Red.	At 33.86° R_2-R_3	Diff. from mean	1°=8.09 div. R_2-G_2	Red.	At 33.86° R_2-G_2	Diff. from mean	1°=8.09 div. R_3-G_2	Red.	At 33.86° R_2-G_2	Diff. from mean
	°	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
1	+ 1.77	—384.3	+198.1	—186.2	—1.5	—507.1	+259.7	—247.4	+1.1	—490.3	+259.7	+230.6	—1.0
2	+ 7.19	349.3	+164.5	184.8	0.1	464.8	+215.7	249.1	—0.7	445.3	+215.7	229.6	+0.0
3	15.52	298.1	+113.2	184.9	0.2	394.4	+148.3	246.1	+2.3	374 8	+148.3	226.5	+3.0
4	22.68	254.0	+ 68.9	185.1	—0.4	334.4	+ 90 4	244.0	+4.4	316.0	+ 90.4	225.6	+4.0
5	27.46	223.2	+ 39.5	183.7	+1.0	304.0	+ 51.8	252.2	—3.8	285.4	+ 51.8	233.6	—4.0
6	30.71	204.6	+ 19.4	185.2	—0.6	264.6	+ 25.5	239.1	+9.3	248.0	+ 25.5	222.5	+7.0
7	33.75	186.0	+ 0.7	185.3	—0.6	250.0	+ 0.9	249.1	—0.7	230.0	+ 0.9	228.7	+0.9
8	36.23	166.8	— 14.6	181.3	+3.3	221.2	— 19.2	240.3	+8.1	201.9	— 19.2	221.1	+8.5
9	38.68	156.8	— 29.7	186.5	—1.8	211.5	— 39.0	250.5	—2.1	191.9	— 39.0	230.9	—1.0
10	41.56	137.1	— 47.5	184.6	+0.1	192.9	— 62.3	255.3	—6.9	174.1	— 62.3	236.4	—6.9
11	45.34	115.2	— 70.9	186.1	—1.4	154.3	— 92.9	247.2	+1.2	134.8	— 92.9	227.7	+1.8
12	48.66	92.1	— 91.3	183.4	+1.2	131.3	—119.7	251.0	—2.6	113.1	—119.7	232.8	—3.3
13	50.93	81.7	—105.3	187.0	—2.3	113.5	— 138.2	251.7	—3.3	94.6	—138.2	232.8	—3.3
14	52.75	68.0	—116.5	184.5	+0.2	102.4	—152.7	255.2	—6.8	82.3	—152.8	535.1	—5.5
15	+54.77	— 52.6	—129.0	—181.6	+3.0	— 79.1	—169.0	—248.0	+0.5	—60.7	—169.0	—229.7	—0.2

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EXHIBIT G.—*Continued.*

No.	T.	1°=6.17 div. R_2-R_3	Red.	At 33°.86 R_2-R_3	Diff. from mean	1°=8.09 div. R_2-G_1	Red.	At 33°.86 R_2-G_1	Diff. from mean	1°=8.09 div R_2-G_5	Red.	At 33°.86 R_2-G_5	Diff. from mean
	°	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.	div.
16	+55.97	−44 3	+110.3	+ 66.0	+1.9	− 67.8	+144.6	+ 76.8	−0.2	− 49.1	+144.6	+ 95.6	+0.2
17	58.20	−32.8	96.5	63.7	−0.3	− 51.8	+126.5	74.7	−2.2	− 33.2	+126.5	93.3	−2.0
18	61.03	−11.5	79.0	67.5	+3.4	− 20.3	+103.6	83.2	+6.2	− 0.9	+103.6	102.7	+7.4
19	63.15	− 3.2	65.9	62.7	−1.4	− 12.8	+ 86.4	73.6	−3.3	+ 5.5	+ 86.4	91.8	−3.5
20	66.73	+18.4	43.9	62.3	−1.7	+ 15.9	+ 57.5	73.4	−3.6	+ 34.7	+ 57.5	92.2	−5.0
21	68.45	+33.7	33.3	67.0	+2.9	+ 36.0	+ 43.7	79.7	+2.8	+ 55.6	+ 43.7	99.3	+4.0
22	72.91	+57.8	+ 5.7	63.5	−0.6	+ 64.7	+ 7.5	72.2	−4.8	+ 83.8	+ 7.5	91.3	−1.0
23	74.76	70.8	− 5.7	65.1	+1.1	+ 87.3	− 7.4	79.9	+3.0	+105.9	− 7.4	98.5	+3.2
24	76.76	82.6	− 18.0	64.6	+0.6	+105.6	− 23.6	82.0	+5 0	+123.9	− 23.6	100.3	+5.0
25	78.68	96.9	− 29.9	67.7	+2.6	+120.4	− 39.2	81.2	+4.3	+137.6	− 39.2	98.4	+3.2
26	81.54	111.5	− 47.5	64.0	−0.1	+139.6	− 62.3	77.3	+0.3	+157.3	− 62.3	95.0	−0.3
27	84.48	128.8	− 65.6	63.2	−0.9	+160.7	− 86.0	74.7	−2.2	+178.3	− 86.0	92.3	−3.0
28	85.92	138.1	− 74.5	63.6	−0.5	+174.9	− 97.7	77.2	+0.2	+192.1	− 97.7	94.4	−0.8
29	88.28	148.1	− 89.0	59.1	−4.9	+190.6	−116.7	73.9	−3.1	+209.6	−116.7	91.4	−3.9
30	+90.69	+166.0	−104.0	+62.0	−2.1	+210.9	−136.3	74.6	−2.4	+229.0	−136.3	+92.7	−2.5

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EXHIBIT H.

Equations of Condition for T=33°.86.

R_2-R_3	R_2-G_1	R_2-G_5
-1.5 div. $=\Delta_a+32.10\Delta_b$	$+1.1$ div. $=\Delta_a+32.10\Delta_b$	-1.0 div. $=\Delta_a+32.10\Delta_b$
$-0.1=\Delta_a+26.67\Delta_b$	$-0.7=\Delta_a+26.67\Delta_b$	$+0.0=\Delta_a+26.67\Delta_b$
$-0.2=\Delta_a+18.34\Delta_b$	$+2.3=\Delta_a+18.34\Delta_b$	$+3.0=\Delta_a+18.34\Delta_b$
$-0.4=\Delta_a+11.18\Delta_b$	$+4.4=\Delta_a+11.18\Delta_b$	$+4.0=\Delta_a+11.68\Delta_b$
$+1.0=\Delta_a+6.40\Delta_b$	$-3.8=\Delta_a+6.40\Delta_b$	$-4.0=\Delta_a+6.40\Delta_b$
$-0.6=\Delta_a+3.15\Delta_b$	$+9.3=\Delta_a+3.15_b\Delta$	$+7.0=\Delta_a+3.15\Delta_b$
$-0.6=\Delta_a+0.11\Delta_b$	$-0.7=\Delta_a+0.11\Delta_b$	$+0.9=\Delta_a+0.11\Delta_b$
$+3.3=\Delta_a-2.37\Delta_b$	$+8.1=\Delta_a-2.37\Delta_b$	$+8.5=\Delta_a-2.37\Delta_b$
$-1.8=\Delta_a-4.82\Delta_b$	$-2.1=+\Delta_a-4.82\Delta_b$	$-1.0=\Delta_a-4.82\Delta_b$
$+0.1=\Delta_a-7.70\Delta_b$	$-6.9=\Delta_a-7.70\Delta_b$	$-6.9=\Delta_a-7.70\Delta_b$
$-1.4=\Delta_a-11.48\Delta_b$	$+1.2=\Delta_a-11.48\Delta_b$	$+1.8=\Delta_a-11.48\Delta_b$
$+1.2=\Delta_a-14.80\Delta_b$	$-2.6=\Delta_a-14.80\Delta_b$	$-3.3=\Delta_a-14.80\Delta_b$
$-2.3=\Delta_a-17.07\Delta_b$	$-3.3=\Delta_a-17.07\Delta_b$	$-3.3=\Delta_a-17.07\Delta_b$
$+0.2=\Delta_a-18.89\Delta_b$	$-6.8=\Delta_a-18.89\Delta_b$	$-5.5=\Delta_a-18.89\Delta_b$
$+3.0=\Delta_a-20.90\Delta_b$	$+0.5=\Delta_a-20.90\Delta_b$	$-0.2=\Delta_a-20.90\Delta_b$
Normal Equations	Normal Equations.	Normal Equations.
$0=1.5\Delta_a-0.08\Delta_b$		
$-83.3=-0.08\Delta_a+3777.3\Delta_b$	$+355.7=3777.3\Delta_b$	$+259.6=1766.6\Delta_b$
$\Delta_b=-0.022\Delta_a$	$\Delta_b=+0.094\Delta_b$	$\Delta_b=+0.088\Delta_b$
b= 6.19 div.	b= 8.00 div.	b= 8.00 div.

Equations of Condition for T=73°.84.

R_2-R_3	R_2-G_1	R_2-G_5
$+1.9$ div. $=\Delta_a+17.87\Delta_b$	-0.2 div. $=\Delta_a+17.87\Delta_b$	$+0.2$ div $=\Delta_a+17.87\Delta_b$
$-0.3=\Delta_a+15.64\Delta_b$	$-2.2=\Delta_a+1.645\Delta_b$	$-2.0=\Delta_a+15.64\Delta_b$
$+3.4=\Delta_a+12.81\Delta_b$	$+6.2=\Delta_a+12.81\Delta_b$	$+7.4=\Delta_a+12.81\Delta_b$
$-1.4=\Delta_a+10.69\Delta_b$	$-3.3=\Delta_a+10.69\Delta_b$	$-3.5=\Delta_a+10.69\Delta_b$
$-1.7=\Delta_a+7.11\Delta_b$	$-3.6=\Delta_a+7.11\Delta_b$	$-3.0=\Delta_a+7.11\Delta_b$
$+2.9=\Delta_a+5.40\Delta_b$	$+2.8=\Delta_a+5.40\Delta_b$	$+4.0=\Delta_a+5.40\Delta_b$
$-0.6=\Delta_a+0.93\Delta_b$	$-4.8=\Delta_a+0.93\Delta_b$	$-4.0=\Delta_a+0.93\Delta_b$
$+1.1=\Delta_a-0.92\Delta_b$	$+3.0=\Delta_a-0.92\Delta_b$	$+3.2=\Delta_a+0.92\Delta_b$
$+0.6=\Delta_a-2.92\Delta_b$	$+5.0=\Delta_a-2.92\Delta_b$	$+5.0=\Delta_a-2.92\Delta_b$
$+2.6=\Delta_a-4.84\Delta_b$	$+4.3=\Delta_a-4.84\Delta_b$	$+3.2=\Delta_a-4.84\Delta_b$
$-1.0=\Delta_a-7.70\Delta_b$	$+0.3=\Delta_a-7.70\Delta_b$	$-0.3=\Delta_a-7.70\Delta_b$
$-0.9=\Delta_a-10.64\Delta_b$	$-2.2=\Delta_a-10.64\Delta_b$	$3.0=\Delta_a-10.64\Delta_b$
$-0.5=\Delta_a-12.08\Delta_b$	$+0.2=\Delta_a-12.08\Delta_b$	$-0.8=\Delta_a-12.08\Delta_b$
$-4.9=\Delta_a-14.44\Delta_a$	$-3.1=\Delta_a-14.44\Delta_b$	$-3.9=\Delta_a-14.44\Delta_b$
$-2.1=\Delta_a-16.85\Delta_b$	$-2.4=\Delta_a-16.85\Delta_b$	$-2.5=\Delta_a-16.85\Delta_b$
$3356.0=3533.2\Delta_b$	$+57.=1766.6\Delta_b$	
$\Delta_b=+0.095$	$\Delta_b=+0.032_b$	$+135.5=1766\ 6_b$
b= 6.07 div.	b= 8.06 div.	$\Delta_b=+0.076$
		b= 8.01 div.

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EXHIBIT I.

Relations between R_2 R_3 and G. in groups of 10 observations each, reduced to 62°.0 Fahr.

62°—T.	R_2—R_3	At 62°.0 R_2—R_3	Mean of groups.
+60°.23	—384.3 div.	—12.7 div.	
54.81	349.3	11.1	
46.48	298.1	11.3	
39.32	254.0	11.4	
34.54	223.2	10.1	—11.32 div.
31.29	204.6	11.5	
28.25	186.0	11.7	
25.77	166.8	7.8	
23.32	156.8	12.9	
20.44	137.1	11.0	—10.98
16.66	115.2	12.4	
13.34	92.1	9.8	
11.07	81.7	13.4	
9.25	68.0	10.9	
7.23	52.6	8.0	—10.90
6.03	44.3	7.1	
3.80	32.8	9.4	
+ 0.97	11.5	5.5	
— 1.15	— 3.2	10.3	
4.73	+ 18.4	10.8	— 8.62
6.45	33.7	1.1	
10.91	57.8	9.5	
12.76	70.8	7.9	
14.76	82.6	8.5	
16.68	96.9	6.0	
19.54	111.5	9 0	
22.48	128.8	9.9	
23.92	138.1	9.5	
26.28	148.1	14.1	
—28.69	+166.0	—11.1	—10.72

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EXHIBIT I.—*Continued.*

Relations between R_2 and G_1.

62°—T.	$R_2—G_1$	At 62.0° $R_2—G_1$	Mean of groups.
+60.23°	—507.1 div.	—19.8 div.	
54.81	464.8	21.4	
46.48	394.4	18.4	
39.32	334.4	16.3	
34.54	304.0	24.5	—20.08 div.
31.29	264.6	11.5	
28.25	250.0	21.5	
25.77	221.2	12.7	
23.32	211.5	22.8	
20.44	192.9	27.5	—19.20
16.66	154.3	19.5	
13.34	131.3	23.4	
11.07	113.5	23.9	
9.25	102.4	27.6	
7.23	179.1	20.6	—23.00
6.03	67.8	19.0	
3.80	51.8	21.1	
+ 0.97	20.3	12.5	
— 1.15	— 12.8	22.1	
4.73	+ 15.9	22.4	—19.42
6.45	36.0	16.2	
20.91	64.7	23.6	
12.76	87.3	15.9	
14.76	105.6	13.8	
16.68	120.4	14.5	—16.80
19.54	139.6	18.5	
22.48	160.7	21.2	
23.91	174.9	18.6	
26.28	190.6	22.0	
—28.69	+210.9	—21.2	—20.30

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EXHIBIT I.—*Continued.*

Relations between R_2 and G_5.

62°—T	R_2—G_5	At 62.0° R_2—G_5	Mean of groups.
+60.23°	—490.3 div.	—3.0 div.	
54.81	445.3	—1.9	
46.48	374.8	+1.2	
39.32	316.0	+2.1	
34.54	284.5	—5.9	—1.50 div.
32.29	248.0	+5.1	
28.25	230.0	—1.5	
25.77	201.9	+6.6	
23.32	191.9	—3.2	
20.44	174.1	—8.7	—0.34
16.66	134.8	+0.0	
13.34	113.1	—5.2	
11.07	94.6	—5.0	
9.25	82.3	—7.5	
7.23	60.7	—2.2	—3.98
6.03	49.1	—0.3	
3.80	33.2	—2.5	
+ 0.97	— 0.9	+6.9	
— 1.15	+ 5.5	—3.8	
4.73	34.7	—3.6	—0.66
6.45	55.6	+3.4	
10.91	83.8	—4.5	
12.76	105.9	+2.7	
14.76	123.9	+4.5	
16.68	137.6	+2.7	+1.76
19.54	157.3	—0.8	
22.48	178.3	—3.6	
23.92	192.1	—1.4	
26.28	209.6	—3.0	
28.69	229.0	—3.1	—2.38

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EXHIBIT J.

Equations between R_2, R_8 and G at 62.0° Fahr.

-11.32 div. $= a + 47.08\Delta_b$
$-10.98 = a + 25.81\Delta_b$
$-10.90 = a + 11.51\Delta_b$
$-8.62 = a + 0.98\Delta_b$
$-7.60 = a - 12.31\Delta_b$
$-10.72 = a - 24.18\Delta_b$

-20.08 div. $= a + 47.08\Delta_b$
$-19.20 = a + 25.81\Delta_b$
$-23.00 = a + 11.51\Delta_b$
$-19.62 = a + 0.98\Delta_b$
$-16.80 = a - 12.31\Delta_b$
$-20.30 = 2 - 24.18\Delta_b$

Normal Equations.

-60.14 div. $= 6a + 48.89\Delta_b$
$-597.47 = 48.89a + 3764.46\Delta_b$
$-10.02 = a + 8.15\Delta_b$
$-12.22 = a + 77.00\Delta_b$
$-2.20 = +68.85\Delta$
$b = -0.033$ div.
$a = -9.78$ div.

Normal Equations.

-19.83 div. $= a + 8.15\Delta_b$
$-21.01 = a + 77.00\Delta_b$
$-1.18 = 68.85\Delta_b$
$\Delta b = -0.17$ div.
$a = -19.70$ div.

$R_2 - G_5$

-1.50 div. $= a + 47.08\Delta_b$
$-0.34 = a + 25.81$
$-3.98 = a + 11.51$
$-0.66 = a + 0.98$
$+1.76 = a - 12.31$
$-2.38 = a - 24.18$

Normal Equations.

-1.18 div. $= a + 8.15\Delta_b$
$-2.14 = a + 77.00\Delta_b$
$-0.94 = 68.85\Delta_b$
$\Delta b = -0.14$ div.
$a = -1.17$ div.

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EXHIBIT K.

Continuation of the comparison between yards R_2, R_3 and G_1.

Thermometer No. 1 rests horizontally upon the upper surface of bar R_2. No. 2 rests upon G. Thermometer No. 3 rests upon the bottom of a slot 22 inches in length, cut in a bronze bar which has the same dimensions as R_2. The depth of the slot is about 0.55 inch. The bulb of the thermometer is completely covered with mercury. The opening in the bar is covered with a thin sheet of rubber, and the pressure of another bar, of the same dimensions upon it excludes the outside air except at the instant of reading. Thermometer No. 4 rests upon the edges of a paper box at the same level as the upper surface of R_2 and about two inches above the bed of the comparator. Thermometer No. 5 is supported in a similar manner at the same level as the upper surface of G.

Date Sept.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_3-G_1	At 62.0° R_2-R_3 plus 9.8 div.	At 62.0° R_2-G_1 plus 1.1 div.
25	6 0	55.9	.....	.....	.....	.....	— 1.7 div.	—51.4 div.	— 53.1 div.	—49.7 div.	— 51.4 div.	— 2.1 div.	— 0.8 div.
25	6 30	55.93	.....	.....	.....	.....	+44.6	— 5.4	— 10.5	—50.0	— 55.1	— 2.6	— 4.8
26	6 10	51.7	.....	.....	.....	.....	+64.8	— 8.6	— 16.0	—73.4	— 80.8	+ 4.0	+ 8.7
26	6 30	51.07	.....	.....	.....	.....	+63.6	— 6.8	— 19.6	—70.4	— 83.2	+ 6.4	+ 5.5
26	12 10	54.1	.....	.....	.....	.....	+52.0	— 4.1	— 13.6	—56.1	— 65.4	+ 2.6	— 0.2
27	6 30	53.2	.....	.....	.....	.....	+54.4	—10.3	— 7.9	- 64.7	— 62.3	— 0.3	+10.2
27	6 50	53.2	.....	.....	.....	.....	+55.9	—13.2	— 10.9	—69.1	— 66.8	— 4.7	+ 5.7
28	6 0	55.25	.....	.....	.....	.....	+ 3.1	—48.1	— 46.0	—51.2	— 49.1	+ 0.4	+ 6.7 *

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EXHIBIT K.

Continuation of the comparisons between yards R_2, R_3 and G_1.

Date Sept.	Time h m	No. 1 °	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	At 62.0° R_2-R_3 plus 9.8 div.	At 62.0° R_1-G_1 plus 1.1 div.
28	6 30	55.25	.....	.....	.....	.....	+ 5.1 div.	—48.1 div.	— 46.0 div.	—53.2 div.	— 51.1 div.	— 1.6 div.	+ 4.7 div.*
28	7 00	55.25	.....	.....	.....	.....	+ 4.1	—48.3	— 50.1	—52.4	— 54.2	— 0.8	+ 1.6 *
28	10 00	55.9	56.0°	.....	.....	.....	— 2.6	—52.6	— 55.1	—50.0	— 52.5	— 2.4	— 2.7 *
28	11 00	56.15	56.45	.....	.....	.....	— 0.9	—51.9	— 59.2	—51.0	— 58.3	— 4.9	—12.2
28	11 30	56.35	56.9	.....	.....	.....	— 3.4	—51.9	— 57.0	—48.5	— 53.6	— 3.7	—11.1
28	12 00	57.05	57.4	.....	.....	.....	— 5.6	—54.1	— 61.2	—48.5	— 55.6	— 8.0	—17.2
28	12 30	57.4	57.9	57.9°	57.9°	.....	— 8.8	—53.8	— 58.0	—45.0	— 49.2	— 6.7	—14.8
28	1 30	58.0	58.1	58.2	58.2	.....	—11.8	—51.8	— 58.0	—40.0	— 46.0	— 5.4	—13.5
28	2 00	58.0	58.2	58.2	58.2	.....	—16.3	—54.2	— 60.4	—37.9	— 44.1	— 3.3	—12.2
28	2 30	58.4	58.85	58.4	58.2	.....	—14.9	—51.6	— 57.7	—36.7	— 42.8	— 4.6	—16.2
28	4 00	58.7	59.15	9.2	59.3	.....	—17.9	—47.4	— 54.0	—29.5	— 36.1	+ 0.8	—11.5 *
28	4 30	58.7	59 0	59.1	59.1	.....	—17.9	—48.1	— 55.1	—30.2	— 37.2	+ 0.1	—11.8 *
28	5 00	59.0	59.1	59.0	59.0	.....	—21.1	—52.2	— 53.0	—31.1	— 31.9	— 2.7	— 7.3 *
29	6 20	54.4	54.8	54.2	54.3	.....	— 2.3	— 54.7	— 54.1	—52.4	— 51.8	+ 4.5	+ 7.7 *
29	6 33	54.5	54.6	54.2	54.3	.....	+ 0.3	—53.2	— 51.2	—53.5	— 51.5	+ 2.8	+ 9.6 *
26	6 49	54.6	54.5	54.1	54.2	.....	+ 0.4	—53.1	— 52.0	—53.5	— 52.4	+ 2.2	+ 9.5 *
29	7 03	54.8	54.4	54.1	54.1	.....	— 2.0	—51.3	— 51.6	—49.3	— 49.6	+ 5.1	+13.1 *
29	7 18	54.8	54.4	54.0	54.0	.....	+ 0.2	—53.1	— 53.1	—53.3	— 53 3	+1 .1 *	+ 9.4 *
29	8 30	54.67	54.15	73.9	53.9	.....	+ 4.3	—54.0	— 53.1	—58.3	— 57.4	— 2.9 *	+ 7.4 *
29	8 40	54.4	54.2	53.9	54.0	.....	+ 2.8	—55.8	— 52.0	—58.6	— 54.8	— 1.7 *	+ 9.4 *

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EXHIBIT K.

Continuation of the comparisons between yards R_2, R_3 and G_1.

Date Sept.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_3-G_1	At 62.0° R_2-R_3 plus 9.8 div.	At 62.0° R_2-G_1 plus 1.1 div.
29	8 50	54.15	54.15	53.8	54.2	.....	+ 0.4 div.	−54.5 div.	− 51.9 div.	−54.9 div.	− 55.3 div.	− 3.6 div. *	+ 9.5 div. *
29	9 00	54.3	54.55	53.85	53.8	.....	+ 0.1	−58.7	− 51.8	−58.8	− 51.9	− 1.3 *	+ 9.6 *
29	10 00	54.1	54.0	53.85	53.85	.....	+ 1.9	−56.6	− 56.3	−58.8	− 58.2	− 0.1 *	+ 7.8 *
29	10 30	54.2	54.15	54.0	54.0	.....	+ 1.7	−57.4	− 57.1	−59.1	− 58.8	− 0.9 *	− 6.0 *
29	10 45	54.5	54.4	54.1	54.1	.....	+ 3.3	−58.5	− 56.8	−61.8	− 60.1	− 5.5 *	+ 2.6 *
29	11 00	54.8	54.8	54.9	54.7	.....	+ 2.9	−54.8	− 57.9	−57.7	− 60.8	− 3.3 *	− 1.3 *
29	11 30	54.8	54.9	54.9	54.85	.....	− 1.2	−58.2	− 60.6	−57.0	− 59.4	− 2.6 *	− 0.7 *
29	12 00	55.0	55.1	55.2	55.1	.....	+ 0.6	−56.9	− 61.5	−57.5	− 62.6	− 4.3 *	− 5.1 *
29	12 20	55.4	55.85	56.1	56.1	.....	+ 1.0	−57.1	− 61.6	−58.5	− 64.9	− 7.4	−11.6 *
29	12 45	55.75	56.1	56.65	56.5	.....	+ 1.0	−58.5	− 63.9	−59.5	− 64.0	−11.0	−16.0 *
29	12 55	Placed hands on R_2, R_3 and G for one minute.											
29	12 59	56.3	56.5	57.0	57.0	.....	−10.6	−63.2	− 74.7	−52.6	− 64.1	− 7.5	−18.4
29	1 15	56.8	56.8	57.2	57.3	.....	− 7.7	−66.8	− 68.5	−59.1	− 60.8	−17.1	−17.5
29	1 55	57.2	57.8	58.0	58.0	.....	− 6.8	−58.2	− 65.0	−51.4	− 58.2	−11.8	−23.0
29	2 30	57.8	58.7	59.4	59.2	.....	−11.9	−60.8	− 60.7	−48.9	− 58.8	−13.1	−30.9
29	3 10	58.6	59.9	60.9	60.7	.....	−14.8	−58.4	− 72.4	−43.6	− 57.6	−12.7	−39.5
29	3 40	59.4	60.8	62.8	61.7	.....	−18.9	−58.5	− 76.9	−39.6	− 58.0	−13.7	−47.2
29	6 00	Built fire in comparing room and in the adjoining room. Replenished fires at 9h P. M. No. 4=81°. No. 3=85°.											
30	6 45	75.9	75.9	71.2	72.1	.....	+ 2.5	+83.4	+143.1	+80.9	+140.6	+ 4.5	+29.0
30	7 00	74.5	73.8	71.35	71.8	.....	+ 5.9	+87.0	+143.0	+81.1	+137.1	+13.4	+44.1

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EXHIBIT K.

Continuation of the comparisons between yards R_2, R_3 and G_1.

Date Sept.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_3-G_1	At 62.0° R_2-R_3 plus 9.8 div.	At 62.0° R_2-G_1 plus 1.1 div.
		°	°	°	°								
30	9 00	70.2	70.2	71.6	72.5	.....	+ 26.8 div.	+87.0 div.	+128.5 div.	+60.2 div.	+101.7 div.	+19.2 div.	+36.3 div.
30	10 00	71.1	70.9	69.7	69.9	.....	+ 34.8	+80.5	+121.7	+45.7	+ 86.9	— 0.9	+14.2
30	11 30	70.65	70.15	69.75	69.85	.....	+ 36.5	+84.0	+120.1	+47.5	+ 83.6	+ 3.7	+18.6
30	12 15	70.45	70.15	69.7	69.7	.....	+ 45.3	+86.8	+123.6	+41.5	+ 78.3	— 1.1 *	+13.3 *
30	1 40	70.0	70.1	69.6	69.6	.....	+ 47.4	+84.3	+112.9	+36.9	+ 65.5	— 2.9 *	+ 0.9 *
30	3 00	69.6	69.6	69.0	69.05	.....	+ 49.6	+86.3	+113.1	+36.7	+ 63.5	— 0.6 *	+ 3.0 *
30	4 00	69.55	69.15	68.9	68.9	.....	+ 49.6	+83.7	+ 99.1	+38.1	+ 49.5	+ 1.1 *	— 7.4 *
Oct.													
2	6 50	59.6	59.65	59.2	59.3	.....	+110.7	+84.3	+ 89.6	—26.4	— 21.1	— 1.7 *	— 1.0 *
2	7 10	59.7	59 7	59.3	59.5	.....	— 4.7	—35.5	— 22.0	—30.8	— 17.3	— 6.7 *	+ 2.4 *
2	7 30	59.9	59.85	59.6	59.8	.....	— 5.4	—35.0	— 26 9	—29 6	— 21.5	— 6.2 *	— 3.0 *
2	7 40	——— Placed both hands on G for one minute— 7^h 41^m. Placed both hands on R_1 for one minute. ———											
2	7 45	59·6	59.85	59.7	59.7	.....	— 14.2	—36.7	— 37.2	—22.5	— 23.0	+ 0.3 *	— 4.5 *
2	7 58	60.0	60.0	60.05	59.9	.....	— 13.4	—34.8	— 33.9	—21.4	— 20.5	+ 0.8 *	— 3.2 *
2	8 45	59.95	59.9	59.8	59.6	.....	— 13.4	—35.5	— 29.9	—22.1	— 16.5	+ 0.4 *	+ 1.6 *
2	12 00	60.8	60.9	60.9	60.9	.....	— 10.0	—33.6	— 29.4	—23.6	— 19.4	— 6.4	— 9 4
2	1 30	61.0	61.1	61.2	61.3	.....	— 10.9	—35.6	— 28.7	—24.7	— 17.8	— 8.7	— 9.4
2	4 00	61.8	71.85	61.9	61.9	.....	— 16.9	— 34.7	— 25.2	—17.8	— 8.3	— 5.8	— 6.0

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EXHIBIT K.

Continuation of the comparisons between yards R_2, R_3 and G_1.

Placed hands upon R_2 for 1 minute at intervals of 5 minutes.

Comparions of R_2 with stops.

	R_2		R_2		R_2		R_2		R_2
h m		h m		h m		h m		h m	
4 46	—0.4 div.	4 51	—10.9 div.	4 56	—21.5 div.	5 1	—31.7 div.	5 6	—36.0 div.	5 11	—40.9 div.
47	—0.2	52	— 9.1	57	—12.1	2	—28.7	7	—35.5		—40.4
48	—1.6	53	— 9.8	58	—19.6	3	—28.6	8	—33.7		—39.5
49	+0.6	54	—11.8	56	—20.3	4	—28.0	9	—33.0		—39.5
Means	—0 5		—10 4		—20.6		- 29.3		—34·6		—40.2

Date Oct.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	At 62.0° R_2-R_2 plus 9.8 div.	At 62.0° R_2-G_1 plus 1.1 div.
3	6 20	60.9°	60.8°	60.7°	60.7	.....	— 1.8 div.	—15.9 div.	— 7.2 div.	—14.1 div.	— 5.4 div.	— 2.5 div.*	+ 5.4 div.*
3	6 40	60.9	60.8	60.4	60.6	.....	+ 0.5	—15.5	— 7.3	—16.0	— 7.8	+ 0.6 *	+ 3.0 *
3	6 50	60.9	60 8	60.5	60.5	.....	+ 0.2	—16.8	— 5.5	—17.0	— 5.7	— 0.4 *	+ 5.1 *
3	7 40	60.95	60.9	60.8	60.7	.....	+ 0.8	—16 9	— 5.9	—17.7	— 6 7	— 1.4 *	+ 3 3 *
3	9 00	61.2	61.15	61 2	61.1	.....	+ 0.4	—17.1	— 9.5	—16.7	— 9.1	— 1.9 *	— 1.1 *
3	10 00	61.8	62.1	62.15	62.0	.....	— 2.3	—18 2	— 9.8	—15.9	— 7.5	— 4.5	— 7.2 *
3	10 30	62.1	62.3	62.8	62.6	.....	— 2 1	—17.8	— 13.6	—15 7	— 11.5	— 6.5	—12.9
3	11 0	62.4	62.8	63.0	62.85	.....	— 3.9	—17.4	— 11.6	—13.5	— 17.7	— 6.2	—13.0
3	12 0	62.8	63.05	63.3	63.1	.....	— 6.8	—18.8	— 10.7	—12.2	— 3.9	— 2.6	—11.4 *
4	6 30	62.95	62.9	63.0	62.9	62.83	—13.7	—17.9	— 2.7	— 4.2	+ 11.0	— 0.3	+ 4.8 *
4	6 40	62.95	62.95	62.9	62.9	62.83	—11.2	—17.3	— 4.5	— 6.1	+ 6.7	— 2.2	+ 0.1 *
4	7 00	63.0	63.0	62.9	62.9	62.83	— 8.9	—17.1	— 4.0	— 8.2	+ 4.9	— 4.4	— 2.1 *

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EXHIBIT K.

Continuation of the comparisons between yards R_2, R_3 and G_1.

Date Oct.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	At 62.0° R_2-R_3 plus 9.8 div	At 62.0° R_2-G_1 plus 1.1 div.
4	9 00	63.25°	63.25°	63.4°	63.25°	63.01°	— 11.4 div.	—18.1 div.	— 7.1 div.	— 6.7 div.	+ 4.3 div.	— 4.1 div.	— 10.2 div.
4	10 00	64.0	64.0	64.1	64.1	63.36	— 14.7	+19.5	— 9.2	— 4.8	+ 5.5	— 7.3	— 9.6
4	10 30	63.95	64.35	64.6	64.6	64.04	— 17.6	—20.8	— 6.5	— 3.2	+ 11.1	— 5.5	— 6.8
4	11 30	64.15	64.7	65.0	94.95	64.22	— 14.8	—20.1	— 5.8	— 5.3	+ 9.0	— 8.8	— 11.8
4	12 30	64.6	64.85	65.0	65.0	64.49	— 18.5	—21.5	— 9.7	— 3.0	+ 8.8	— 9.3	— 13.2
4	2 20	54.85	65.05	65.15	65.2	64.78	— 23.0	—20.2	— 8.1	+ 2.8	+ 14.9	— 5.1	— 8.7
4	3 50	65.0	65.2	65.1	65.35	64.99	— 23.9	—15.2	— 3.0	+ 8.7	+ 20.9	— 0.1	— 3.9 *
4	4 20	65.1	65.15	65.15	65.3	65.12	— 24.9	—17.5	— 2.7	+ 7.4	+ 22.2	— 2.0	— 2.2 *
4	5 00	65.1	65.15	65.2	65.0	65.16	— 23.4	—14.4	— 0.4	+ 9.0	+ 23.0	— 0.4	— 1.4 *
5	1 45 PM	62.7	62.6	62.5	62.7	62.46	— 10.6	—18.3	— 4.9	— 7.7	+ 5.7	— 2.3	— 2.0 *
5	2 00	62.7	62.7	62.65	62.75	62.50	— 9.6	—18.9	— 9.2	— 9.3	+ 0.4	— 3.8	— 4.2 *
5	2 20	62.7	62.8	62.85	62.85	62.53	— 9.6	—16.9	— 8.0	— 7.3	+ 1.6	— 1.8	— 3.7 *
5	2 40	62.65	62.8	62.7	62.85	62.55	— 12.9	—19.8	— 10.7	— 6.9	+ 2.2	— 1.1	— 3.1 *
5	3 00	62.65	62.8	62.8	62.8	61.55	— 11.3	—19.3	— 7.2	— 8.0	+ 4.1	— 2.2	— 1.2 *
5	3 15	62.8	62.9	62.7	62.9	62.60	— 12.1	—20.0	— 6.9	— 7.9	+ 5.2	— 3.1	— 1.0 *
5	3 30	62.8	62.8	62.6	62.7	62.62	— 15.1	—19.9	— 8.5	— 4.8	+ 6.6	+ 0.0	+ 1.3 *
5	3 45	62.8	62.8	62.7	62.8	62.64	— 15.5	—20.1	— 8.4	— 4.6	+ 7.2	+ 0.2	+ 1.8 *
5	3 52	Placed one hand on G. and the other on R_2 and R_2 for one minute.											
5	3 35	63.0	62.7	.....	.....	.....	— 17.2	—23.5	— 12.3	— 6.3	+ 4.9	— 2.7	+ 0.3
5	4 00	63.0	62.8	.....	.....	.....	— 18.8	—25.0	— 14.8	— 6.2	+ 4.8	— 2.6	— 0.5
5	4 05	62.8	62.9	.....	.....	.....	— 18.0	—24.5	— 14.0	— 6.5	+ 4.0	— 1.7	— 2.2

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EXHIBIT K.

Continuation of the comparisons between yards R_2, R_3 and G_1.

Date Oct.	Time h	m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	At 62.0° R_2-R_3 plus 9.8 div.	At 62.0° R^5-G_1 plus 1.1 div.
5	4	12	62.8°	62.9°	.....	.....	.....	— 16.0 div.	—22.0 div.	— 10.4 div.	— 6.0 div.	+ 5.6 div.	— 1.2 div.	— 0.6 div.
5	4	20	62.8	62.9	.....	.....	.....	— 16.5	—23.5	— 9.0	— 7.0	+ 7.5	— 2.2	+ 1.3
5	3	30	62.8	62.9	.....	.....	.....	— 16.4	—23.0	— 12.0	— 6.6	+ 4.4	— 1.8	— 1.8
5	4	32	....	....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
Placed hands on R_2 for 1 minute at intervals of 5 minutes.														
5	4	33	....	....	.....	.....	.....	— 21.7	.....	.....	.....	.....	.....	.....
5	4	38	....	....	.....	.....	.....	— 28.7	.....	.....	.....	.....	.....	.....
5	4	43	....	....	.....	.....	.....	— 31.3	.....	.....	.....	.....	.....	.....
5	4	48	....	....	.....	.....	.....	— 35.0	.....	.....	.....	.....	.....	.....
5	4	53	....	....	.....	.....	.....	— 37.7	.....	.....	.....	.....	.....	.....
5	4	58	....	....	.....	.....	.....	— 41.9	.....	.....	.....	.....	.....	.....
6	7	10	59.2	59.1	59.1°	58.8°	59.43°	+ 8.0	—19.3	— 8.0	—27.3	— 16.0	— 0.1	+ 8.5 *
6	7	20	59.2	59.1	58.9	58.9	59.43	+ 6.6	—19.7	— 10.2	—26.3	— 16.8	+ 0.9	+ 7.8 *
6	8	10	59.5	59 5	59.0	59.0	59.59	+ 7.0	—18.6	— 13.2	—25.6	— 20.2	— 0.3	+ 1.2 *
6	8	30	60.1	60.15	60.15	60.15	59.81	+ 7.0	—18.5	— 14.5	—25.5	— 21.5	— 3.9	— 5.4 *
6	9	00	60.7	60.9	61.0	61.1	59.81	+ 7.2	—19.4	— 16.1	—26.6	— 23.3	— 8.7	—13.3 *
6	10	30	61.0	61.5	61.8	62.0	60.62	+ 3.0	—17.8	— 21.5	—20.8	— 24.5	— 4.8	—19.4 *
6	10	45	61.6	61.95	62.25	62.3	60.87	+ 2.2	—18.0	— 21.6	—20.2	— 23.8	— 7.9	—22.3
6	12	0	61.75	62.0	62.5	62.7	61.47	— 0 8	—20.9	— 19.8	—20.1	— 19.3	— 8.7	—18.2
6	4	15	62.8	62.85	63.15	63.0	62.64	— 9.9	—17.8	— 21.4	— 7.9	— 11.5	— 3.1	—17.3
6	4	25	Opened both windows. Temperature of outside air = 56°											
6	4	40	62.5	62.7	62.4	62.8	62.60	— 9.5	—18.2	— 9.6	— 8.7	+ 0.8	— 2.0	— 3.8
6	5	10	62.0	62.0	61.5	61.9	62.24	— 9.1	—17.6	— 1.8	— 8.5	+ 7.3	+ 1.3	+ 8.4
6	8	0	60.9	60.5	59.2	59.7	60.17	— 1.9	—18.8	— 6.1	—16.9	+ 4.2	— 0.3	+ 9.1

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EXHIBIT L.

Comparison between metres R_2 R_3 and G_1.

Date	Time	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	At 62.0° R_2-R_3 plus 48 div.	At 62.0° R_2-G_1 minus 140 div.
Oct.	h m	°	°	°	°								
10	6 30	59.15	59.2	59.3	59.3	59.23	+ 3.7 div.	−17.1 div.	− 5.7 div.	−20.8 div.	− 8.4 div.	+ 3.3 div.	+ 2.4 div.
10	9 00	59.25	59.35	59.3	59.5	59.19	+ 4.0	−17.2	−5.0	−21.2	−9.0	+ 2.2	+ 0.5
11	6 20	61.75	61.45	61.0	61.0	61.50	− 13.3	−18.6	− 6.0	− 5.3	+ 7.3	+ 1.2	− 1.8
11	6 40	61.5	61.4	60.5	60.6	61.52	− 10.3	−19.1	− 5.3	− 8.8	+ 5.0	− 0.6	− 3.7
11	9 30	61.75	61.9	62.1	62.1	61.56	− 11.1	−17.3	− 5.2	− 6.2	+ 5.9	+ 0.3	− 7.2
11	11 45	61.8	61.65	61.6	61.6	61.70	− 10.9	−19.1	− 4.4	− 8.2	+ 6.5	− 2.0	− 4.5
11	3 20	61.15	61.0	60.8	60.9	61.23	+ 1.8	− 6.1	+ 3.7	− 7.9	+ 1.9	+ 2.7	− 3.2
11	3 40	61.1	61.0	61.0	61.0	61.16	− 2.7	− 9.0	+ 4.4	− 6.3	+ 7.1	+ 4.6	+ 2.0
11	4 00	61.0	60.9	60.7	60.7	61.00	+ 0.1	−10.0	+ 4.0	−10.1	+ 4.1	+ 1.5	− 0.2
12	6 25	53.7	53.2	53.0	53.1	53.42	+ 61.5	+ 6.3	+ 9.1	−55.2	− 52.4	+ 5.9	+11.7
12	6 50	33.85	53.25	53.0	53.1	53.46	+ 61.8	+ 8.6	+ 5.4	−53.2	− 56.6	+ 6.8	+ 7.0
12	8 00	54.0	53.95	53.8	53.7	53.40	+ 61.4	+ 8.8	+ 0.6	−52.6	− 60.8	+ 6.4	− 3.4
12	9 10	53.6	53.6	53.05	3.0	53.38	+ 61.8	+ 6.1	− 1.8	−55.7	− 63.6	+ 6.0	− 3.1
12	10 30	55.5	55.85	56.0	56.0	55.45	+ 56.4	+ 7.1	+ 0.7	−49.3	− 55.7	− 0.4	−15.2
12	11 15	55.9	56.1	56.0	56.0	56.12	+ 57.9	+ 5.9	+ 2.7	−52.0	− 55.2	− 5.9	−16.9
12	11 40	56.15	56.45	56.3	56.4	56.56	+ 58.0	+ 4.9	+ 5.1	−53.1	− 52.9	− 8.6	−17.7
12	11 55	Removed R_2 R_3 and G to open air exposure											

At 1^h 35^m, temperature by No. 1, air contact =44.5° and by No. 2 by contact with R_2=45.7°.
Removed G. R_2 and R_3 to comparator.

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EXHIBIT L.

Continuation of the comparisons between meters R_2, R_3 and G_1.

Date Oct.	Time h	m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	R_2-R_3	R_2-G_1
12	2	15	53.0°	53.55°	....	....	.....	+104.5 div.	+41.7 div.	+ 40.3 div.	−62.8 div.	− 64.2 div.	+ 3.0 div.	− 3.3 div.
12	2	30	54.7	54.8	....	....	.....	+ 97.2	+33.1	+ 25.9	−64.1	− 73.1	− 9.8	−21.4
12	2	45	54.7	55.1	....	....	56.62°	+ 79.6	+34.0	+ 20.3	−45.6	− 59.3	+ 8.7	−12.1
12	3	00	55.1	55.7	....	....	56.66	+ 73.5	+18.1	+ 17.1	−55.4	− 56.4	− 3.8	−14.5
12	3	15	55.6	55.9	....	....	56.66	+ 62.9	+12.6	+ 14.2	−50.3	− 48.7	− 2.1	− 8.6
12	3	35	55.7	55.9	...	....	56.66	+ 59.4	+ 8.7	+ 13.4	−50.7	− 46.0	− 3.2	− 5.9
12	2	30	55.8	55.75	....	....	56.34	+ 54.0	+ 4.0	+ 14.7	−50.0	− 39.3	− 3.2	+ 2.1
12	5	10	55.6	55.4	....	....	56.05	+ 53.0	+ 8.5	+ 10.3	−44.5	− 42.7	+ 3.7	+ 1.8
13	10	15	53.9	53.95	53.9°	53.8°	34.00	+ 65.0	+11.1	+ 12.5	+53.9	− 52.5	+ 5.8	+ 4.9
12	11	00	53.8	53.5	52.8	53.0	53.67	+ 63.9	+ 9.6	+ 9.8	−54.3	− 54.1	+ 6.1	+ 7.3
13	11	30	53.85	53 5	52.8	53.0	53.62	+ 63.9	+ 3.3	+ 8.8	−60.6	− 55.1	− 0.6	+ 6.3
13	12	00	53.7	53.55	53.0	53.05	53.60	+ 61.5	+ 1.9	+ 5.9	−59.6	− 55.6	+ 1.5	+ 5.3
13	12	30	54.0	54.0	53.9	54.0	53.67	+ 62.3	+ 2.5	+ 5.0	−59.8	− 57.3	− 0.8	− 0.4
13	1	30	53.13	54.85	55.0	54.85	54.32	+ 61.1	+ 4.8	+ 0.6	−56.3	− 60.5	− 4.9	−11.1
13	2	00	57.0	57.1	56.1	55.85	55.99	+ 49.9	+ 3.1	− 1.9	−46.8	− 51.8	− 8.1	−22.3
13	2	20	———	———	Opened both windows. Closed windows at 2^h 45^m Outside air = 54.0°.									———
13	3	00	56.1	56.7	57.0	57.0	.....	+ 54.2	+ 4.8	− 1.1	−49.4	− 55.3	− 4.6	−22.3
14	—	—	———	———	Both windows open during the night of Oct. 13th. Closed at 6^h 15^m Oct. 14th.									———
14	6	10	43.9	43.5	41.8	41.8	43.88	+ 12.77	+ 4.9	− 7.7	−122.8	−135.4	+ 4.7	−14.7
14	6	40	45.0	45.0	44.3	44.1	43.79	+ 12.68	+ 4.7	− 14.6	− 122.1	−141.4	− 1.5	− 4.6
41	7	40	46.7	47.5	48.2	48.1	.....	+ 12.24	+ 6.3	− 18.0	−116.1	−140.4	− 7.6	−25.8

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EXHIBIT L.

Continuation of the comparisons between meters R_2, R_3 and G_1.

The reductions are made from the readings of thermometers Nos. 1 and 2 with the relative coefficient 6.78 div. for R_2-R_3 and 8.87 div. for R_2-G_1.

Date	Time	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	R_3-R_3	R_2-G_1
	h m	°	°	°	°	°							
14	9 20	49.4	50.4	52.0	51.9	48.70	+106.8 div.	+ 6.7 div.	— 25.4 div.	—100.1 div.	—132.2 div.	—11.9 div.	—35.0 div.
14	10 00	49.9	51.0	53.0	53.0	48.82	+103.8	+ 9.7	— 18.6	— 93.1	—121.4	— 6.2	—37.8
14	10 10	sky overcast.											
14	12 30	55.0	55.4	53.85	52.95	53.06	+ 83.2	+ 8.6	— 7.1	— 74.6	— 90.3	—22.3	—45.8
14	2 15	54.6	54.9	56.2	56.05	54.48	+ 73.3	+ 6.4	— 3.2	— 66.9	— 76.5	—11.9	—27.6
14	3 40	54.95	55.5	56.2	56.2	55.26	+ 64.1	+ 6.2	— 2.6	— 57.9	— 66.7	— 5.3	—23.1
14	4 50	55.45	55.8	56.25	56.2	55.70	+ 58.3	+ 6.4	— 1.5	— 51.9	— 59.8	— 2.7	—18.8
16	6 10	45.9	45.65	44.8	44.8	45.86	+112.7	+ 2.5	— 4.7	—110.2	—117.4	+ 3.7	+13.6
16	6 40	46.1	45.85	45.1	45.2	45.73	+114.3	+ 4.1	— 4.3	—110.2	—118.6	+ 2.4	+10.6
16	7 20	46.0	46.0	45.5	45.5	45.82	+116.6	+ 3.7	— 5.1	—112.9	—121.7	+ 0.4	+ 6.2
16	1 40	46.35	46.35	46.0	46.1	45.95	—117.9	+ 4.9	— 8.6	—113.0	—126.5	— 2.1	— 1.7
16	9 30	47.00	47.00	47.0	47.05	46.33	+115.3	+ 7.4	— 11.6	—107.9	—127.2	— 1.4	— 8.2
16	10 00	47.75	47.95	48.5	48.4	46.99	+114.1	+ 5.1	— 15.3	—109.9	—129.4	— 8.5	—18.8
16	11 00	49.0	49.9	51.3	51.1	48.65	+105.8	+ 5.0	— 12.9	—100 8	—118.9	— 7.9	—25.6
16	12 00	50.25	51.2	53.0	52.9	50.36	+ 99.8	+ 5.9	— 14.8	— 93.9	—114.6	— 9.5	—32.8
16	1 50	52.1	52.9	54.3	54.2	53.20	+ 85.5	+ 6.5	— 8.8	— 79.0	— 94.3	— 7.1	—27.6
16	3 45	53.5	54.0	54.95	55.05	53.74	+ 76.6	+ 7.4	— 0.8	— 69.2	— 77.4	— 6.8	—20.4
16	5 10	54.0	54.5	55.1	55.0	54.32	+ 68.7	+ 3.0	+ 4.1	— 65 7	— 64.6	— 6.7	—12.1
17	6 20	51.25	51.15	54.5	50.9	51.31	+ 83.6	+ 5.1	+ 10.4	— 78.5	— 73 2	— 0 9	+ 9.2
17	6 50	51.05	51.00	50.5	50.8	51.26	+ 83.8	+ 6.5	+ 9.2	— 77.3	— 74.6	+ 1.7	+ 9.0

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EXHIBIT L.

Continuation of the comparisons between meters R_2, R_3 and G_1.

Date Oct.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1	R_3-R_2	R_3-G_1
17	7 50	51°.05	50°.95	50°.7	50°.8	50°.89	+ 83.5 div.	..... div.	+ 8.2 div.	..... div.	— 75.2 div.	.....	+ 8.8 div.
17	9 20	51.20	51.25	51.2	51.2	50.93	+ 82.8	.....	+ 4.5	.....	— 78.3	.....	+ 3.0
18	6 50	55.6	55.8	55.8	55.9	55.27	— 3.7	—57.3	— 55.6	— 53.6	— 51.9	— 5.4 div.	—10.9
18	7 20	55.5	55.6	55.6	55.9	55.42	— 5.6	—60.0	— 53.9	— 54.4	— 48.3	— 5.5	— 5.5
25	12 30	55.9	.....	56.3	....	55.31	+ 8.4	+58.9	.....	— 50.5	.....	— 4.3	
25	4 45	55.45	.....	55.0	....	55.80	+ 18.0	+71.4	.....	— 53.4	.....	— 4.2	
25	9 00	53.2	.....	53 0	....	54.05	+ 30.9	+92.8	.....	— 61.9	.....	+ 2.6	
27	6 10	46.95	.....	46.95	....	46.85	+140.9	+32.8	.....	—108.1	.....	— 1.3	
	6 20	46.95	.....	46.95	....	46.82	+140.0	+32.0	.....	—108.0	.....	— 1.2	
	6 30	46.9	.....	46.9	....	46.80	+137.1	+30.3	.....	—106.8	.....	+ 0.4	
	6 40	46.9	.....	46.9	....	46.78	+139.1	+29.6	.....	—109.5	.....	— 2.3	
	6 50	46.9	.....	46.8	....	46.77	+140.5	+31.2	.....	—109.3	.....	— 2.1	
	7 00	46.8	.....	46.8	....	46.76	+139.9	+32.1	.....	—107.8	.....	+ 0 0	
	7 10	46.8	.....	46.8	....	46.76	+139.3	+29.1	.....	—110.2	.....	— 2.4	
	7 20	46.8	.....	46.8	....	46.75	+139.4	+30.9	.....	—108.5	.....	— 0.7	
	8 15	46.05	.....	45.95	....	46.69	+140.4	+28.2	.....	—112.3	.....	+ 0.6	
	8 20	46.1	.....	46.0	....	46.68	+140.0	+27.0	.....	—113.0	.....	— 0.4	
	8 25	46.1	.....	46.0	....	46 65	+140.8	+28.4	.....	—112.4	.....	+ 0.2	
	8 50	46.3	.....	46.15	....	46.63	+141.6	+25.9	.....	—115.7	.....	— 4.5	
	8 35	46.3	.....	46.15	....	46.63	+140.7	+23.4	.....	—117.3	.....	— 6.1	
	9 30	46.6	.....	46.3	....	46.71	+136.5	+23.3	.....	—113.2	.....	— 4.0	
	9 35	46.6	.....	46.3	....	46.71	+135.6	+21.5	.....	—114.1	.....	— 4.9	

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EXHIBIT L.

Continuation of the comparisons between meters R_2, R_3 and G_1.

Date Nov.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1
3	6 30	59.9°	58.9°	58.3°	....	61.39°	+ 24.4 div.	+18.0 div.	+51.1 div.	+ 12.6 div.	+40.2 div.
3	6 55	57.8	57.8	57.0	....	60.89	+ 34.3	+19.1	+47.8	+ 11.3	+36.7
3	7 10	57.9	57.9	57.0	....	60.51	+ 38.0	+20.5	+48.4	+ 13.1	+39.0
3	8 05	58 0	57.0	56.7	....	59.47	+ 45.1	+18.8	+45.0	+ 5.6	+30.1
3	9 10	57.9	57.2	57.5	....	58.62	+ 45.1	+15.4	+38.7	+ 2.9	+22.2
3	9 35	57.9	57.7	58.0	....	58.64	+ 46.7	+13.0	+30.6	— 0.1	+ 7.6
3	10 00	58.3	58.05	58.1	....	58.64	+ 43.1	+ 9.9	+24.2	— 3.3	+ 1.5
3	10 25	58.1	58.1	58.1	....	58.62	+ 41.7	+ 9.2	+22.9	— 1.3	+ 1.8
3	10 40	58.2	58.2	58.2	....	58.60	+ 42.4	+ 8.3	+23.6	— 3.5	+ 0.9
3	10 55	58.4	58.4	58.6	....	58.58	+ 42.0	+ 9.7	+24.5	— 3.1	+ 0.5
3	11 10	58.5	58 4	58.4	....	58.62	+ 41.3	+ 8.9	+20.8	— 3.9	— 2.5
3	11 30	58.6	58.8	58.7	....	58.68	+ 36.7	+ 5.5	+16.9	— 3.4	— 5.4
3	11 50	58.7	58.9	58.7	....	58.70	+ 37.6	+ 6.3	+20.0	— 4.1	— 4.1
3	12 10	58.9	59.0	59.0	....	58.72	+ 38.2	+ 4.2	+20.2	— 8.2	— 5.4
3	12 13	———	Opened	windows	in the	partition	between	rooms.	Temperature	in adjoining	room =98°———
3	12 20	60.4	61.2	61.3	....	59.25	+ 33.8	+ 7.4	+ 8.7	— 10.5	—32.1
3	1 15	63.2	63.4	64.2	....	60.22	+ 20.1	— 1.0	— 1.1	— 21.7	—50.3
3	1 35	64.5	64.9	65.6	....	60.58	+ 12.0	— 3.8	— 4.8	— 27.9	—56.5
3	1 50	64.9	66.3	66.3	....	61.21	+ 8.8	— 6.3	— 5.5	— 30.6	—67.3
3	5 00	62.9	62.9	62.7	....	62.33	+ 2.2	— 0.9	.....	— 6.3	.....
3	5 45	63.0	62.9	62.9	....	62.53	+ 3.5	+ 0.8	.....	— 5.9	.....
3	6 40	61.8	61.7	61.2	....	62.06	+ 5.8	+ 2.2	.....	+ 3.9	.....

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EXHIBIT L.

Continuation of the comparisons between meters R_2, R_3 and G_1.

Date Nov.	Time h m	No. 1	No. 2	No. 3	No. 4	No. 5	R_2	R_3	G_1	R_2-R_3	R_2-G_1
3	7 20	61.°1	61.°0	61.°0	....	61.°70	+ 10.9 div.	+ 6.3 div.	.....	+ 9.1 div.	.....
5	9 50	59.1	58.9	58.9	....	60.04	+ 24.8	+ 6.3	.....	+ 13.8	.....
3	10 05	58.8	58.6	58.8	....	59.92	+ 28.3	+ 6.8	.....	+ 13.5	.....
3	10 50	58.7	58.2	58.1	....	59.50	+ 33.6	+ 9.0	.....	+ 4.4	.....
3	11 30	58.7	58.1	58.1	....	59.13	+ 36.0	+ 7.4	.....	— 0.1	.....
2	12 00	58.4	57.9	58.6	....	58.78	+ 36.3	+ 6.9	.....	— 0.2	.....
3	12 30	58.9	58.7	58.6	....	58.95	+ 36.2	+ 5.9	.....	+ 3.8	.....

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EXHIBIT M.

Experiment 1.

Change in length of R_3 in passing from 34°.05 to 64°.1.
Removal to comparator at 7^h 10^m.

The values of T in the second column relate to the temperatures of the bar of metal after removal to the comparator. The values in the third column relate to the air temperature of the comparing room.

Time h m	T °	T °	Div.				
7 35	....	64.5	+218.2*				
7 51	58.1	64.3	+150.7				
				−160.9			
8 7	59.0	64.5	+100.3				
8 23	59.4	64.3	+ 57.3*		+68.7		
8 39	59.5	64.2	+ 19.1				
				− 92.2		−30.3	
8 55	59.5	64.4	− 10.7				
9 11	59.4	64.5	− 34.9*		+38.4		+ 8.9
9 27	60.9	64.6	− 54.7				
				− 53.8		−21.4	+10.4
9 43	6.15	64.5	− 72.1				
9 59	62.1	64.8	− 88.7*	.....	+17.0	.....	+19.3 ..... −25.6
10 15	63.0	64.8	−102.9*				
				− 36.8		− 2.1	−15.2
10 31	63.6	64.7	−116.5				
10 47	64.1	64.7	−125.5*		+14.9		+ 4.1
11 3	64.0	64.7	−132.7				
				− 21.9		+ 2.0	
11 41	63.6	64.0	−142.8				
11 57	63.6	64.0	−147.4*		+16.9		
12 13	....	....	.....				
				− 5.0			
12 29	....	....	.....				
12 45	64.1	64.1	−152.4*				

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EXHIBIT M.— *Continued.*

Experiment 2.

Change in length of R_2 in passing from 35°.05 to 50°.2.
Removal to comparator at $9^h\ 43^m$.

Time h m	T °	T °	Div.				
10 0	44.2	49.2	+159.7*				
10 16	43.6	49.4	+115.2				
				−106.9			
10 32	44.6	49.5	+ 80.2				
10 48	45.7	49.4	+ 52.8*		+54.6		
11 4	46.4	49.6	+ 31.8				
				− 52.3		−30.1	
11 20	47.2	49.7	+ 14.6				
11 36	47.6	49.7	+ 0.5*		+24.5		+17.9
11 52	48.0	49.7	− 8.9				
				− 27.8		−12.2	− 8.9
11 8	48.2	49.2	− 18.0				
12 24	48.8	49.7	− 27.3*	.....	+12.3	.....	+ 9.0 ..... + 0.4
12 40	48.9	49.7	− 31.0				
				− 15.5		− 3.2	− 8.5
12 58	49.2	49.8	− 38.2				
1 14	49.5	50.1	− 42.8*		+ 9.1		+ 0.5
1 30	49.8	50.1	− 47.4				
				− 6.4		− 2.7	
1 45	49.9	50.0	− 49.5				
2 15	49.5	49.4	− 49.2*		+ 6.4		
2 31	....	....	.....				
				− 0.0			
2 47	....	....	.....				
3 03	50.1	50.1	− 49.2*				

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EXHIBIT M.— *Continued.*

Experiment 3.

Change in length of R_2 in passing from 59.05° to 56.8°.
Removal to comparator at 1h 50m.

Time h m	T °	T °	Div.							
2 15	57.7	56.2	+ 12.3*							
2 31	57.7	56.3	20.7	+ 12.9						
2 47	57.5	56.7	25.2*		− 2.3					
3 3	57.3	56.7	30.1	+ 10.6		− 0.9				
3 19	57.2	56.7	35.8*		− 3.2		− 1.0			
3 35	57.1	56.8	39.8	+ 7.4		− 1.9		− 7.9		
3 51	57.0	56.7	43.2*	.....	− 5.1	.....	+ 6.9	.....	−21.6	
4 7	57.0	56.8	44.7	+ 2.3		+ 5.0		−13.7		
4 23	56.9	56.7	45.5*		− 0.1		− 6.8			
4 39	56.9	56.7	47.3	+ 2.2		− 1.8				
4 55	57.0	56.7	47.7*		− 1.9					
5 11	57.0	56.8	48.0	+ 0.3						
5 27	56.8	56.6	+48.0*							

Experiment 4.

Change in length of R_2 in passing from 68.0° to 64.1°.
Removal to comparator at 2h 26m.

h m	°	°								
2 41	65.1	64.6	−201.7*							
2 57	66.0	65.0	−188.8	+ 23.9						
3 13	65.1	65.1	−177.8*		−13.9					
3 29	64.6	65.0	−171.0	+ 10.0		+ 6.5				
3 45	64.4	64.8	−167.8*		− 7.4		+ 0.5			
4 1				+ 2.6		+ 7.0		−7.7		
4 17	64.2	64.4	−165.2	.....	− 0.4	.....	− 7.2	.....	+12.6	
4 23				+ 2.2		− 0.2		+4.9		
4 49	64.3	64.9	−163.0*		− 0.6		− 2.3			
5 5				+ 1.6		− 2.5				
5 21	64.7	64.6	−161.4		− 3.1					
5 37				− 1.5						
5 53	64.1	63.9	−162.9*							

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EXHIBIT M.— *Continued.*

Experiment 5.

Change in length of R_3 in passing from 34°.05 to 64°.1.
Removal to comparator at 7^h 10^m.

Time h m	T °	T °	Div							
7 39	....	64.5	+ 64.2*							
7 55	58.1	64.3	+ 21.3							
				−100.9						
8 11	56.1	64.5	− 10.3							
8 27	56.1	64.2	− 36.7*		+48.8					
8 43	58.5	64.3	− 58.9							
				− 52.1		−24.4				
8 59	59.0	64.5	− 77.3							
9 15	59.4	64.7	− 88.8*		+24.4		+10.1			
9 31	60.9	64.7	− 98.7							
				− 27.7		−14.3		+ 1.8		
9 47	62.1	64.5	−107.4							
10 3	62.5	64.8	−116.5*	.....	+10.1	.....	+11.9	.....	−10.1	
10 19	62.6	64.7	−122.4							
				− 17.6		− 2.4		− 8.3		
10 36	63.0	64.6	−129.9							
10 51	64.0	64.8	−134.1*		+ 7.7		+ 3.6			
11 7	64.2	64.9	−140.4							
				− 9.9		+ 1.2				
11 23	64.1	64.7	−142.3							
11 39	64.1	64.7	−144.0*		+ 8.9					
11 55		....								
				− 1.0						
12 11		....								
12 27	64.1	64.7	145.0*							

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EXHIBIT M.— *Continued.*

Experiment 6.

Change in length of R_s in passing from 32°.05 to 49°.7.
Removal to comparator at $9^h\ 43^m$.

Time h m	T °	T °	Div.				
9 50	44.0	49.2	+ 6.2*				
10 6	43.3	49.2	— 34.8				
				— 68.7			
10 22	44.0	49.9	— 56.0				
10 38	44.8	49.4	— 74.9*		+33.6		
10 54	46.1	49.5	— 91.2				
				— 35.1		—15.2	
11 10	46.9	49.5	—103.0				
11 26	47.4	49.7	—110.0*		+18.4		+ 3.8
11 42	47.8	49.7	—115.0				
				— 16.7		—11.4	+ 5.1
11 58	48.1	49.5	—120.7				
12 14	48.5	49.4	—126.7*	.....	+ 7.0	..... + 8.9	..... —11.8
12 30	48.9	49.8	—130.4				
				— 9.7		— 2.5	— 6.7
12 46	49.1	49.7	—133.5				
1 2	49.4	49.0	—136.4		+ 4.5		+ 2.2
1 18	49.8	50.0	—139.6				
				— 5.2		— 0.3	
1 34	49.9	50.0	—142.7				
2 20	49.7	49.6	—141.6*		+ 4.2		
2 36	....	....	.....				
				— 1.0			
2 52	....	....	.....				
3 08	49.9	49.6	142.6				

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EXHIBIT M.— *Continued.*

Experiment 7.

Change in length of R_s in passing from 59°.05 to 56°.6.
Removal to comparator at $1^h\ 59^m$.

Time h m	T °	T °	Div.								
2 8	....	56.1	— 27.6*								
2 24	57.7	56.2	— 19.5	+ 12.6							
2 40	57.6	56.4	— 15.0*		— 6.1						
2 56	57.6	56.6	— 11.3	+ 6.5		+ 5.7					
3 12	57.2	56.7	— 8.5*		— 0.4		— 7.5				
3 28	57.2	56.7	— 5.8	+ 6.1		— 1.8		+ 7.9			
3 44	57.1	59.8	— 2.4*	.....	— 2.2	.....	+ 0.4	.....	— 1.7		
4 0	57.0	56.8	— 0.8	+ 3.9		— 1.4		+ 6.2			
4 16	56.9	56.8	+ 1.5*		— 3.6		+ 5.8				
4 32	56.9	56.7	+ 0.6	+ 0.3		+ 4.4					
4 48	56.9	56.7	+ 1.8*		+ 0.8						
5 4	56.8	56.7	+ 2.9	+ 1.1							
5 20	56.6	56.5	+ 2.9*								

Experiment 8.

Change in the length of R_s in passing from 68°.0 to 64°.1.
Removal to comparator at $2^h\ 26^m$.

h m	°	°									
2 49	65.1	64.6	—175.7*								
3 1	65.7	65.2	—170.6	+ 10.3							
3 17	65.1	65.2	—165.4*		— 5.1						
3 33	64.8	64.9	—162.1	+ 5.2		+ 1.7					
3 49	64.4	64.8	—160.2*		— 3.4		+ 1.8				
4 5				+ 1.8		+ 3.5		— 7.0			
4 21	64.2	64.3	—158.4*	.....	+ 0.1	.....	— 5.2	.....	+14.3		
4 37				+ 1.9		— 1.7		+ 7.3			
4 53	64.6	64.7	—156.5		— 1.6		+ 2.1				
5 9				+ 0.3		+ 0.4					
5 25	64.7	64.6	—156.2*		— 1.2						
				— 0.9							
5 58	61.1	63.9	—157.1*								

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EXHIBIT M.— *Continued.*

Experiment 9.

Change in length of G in passing from 34°.05 to 63°.6.
Removal to comparator at 7^h 10^m.

Time h m	T °	T °	Div.				
7 31	....	....	+ 15.6*				
7 47	58.2	64.5	— 29.0				
				— 92.5			
8 3	58.6	64.4	— 58.5				
8 17		64.3	— 76.9*		+56.7		
8 35	60.2	64.2	— 91.8				
				— 35.8		—34.9	
8 51	60.1	64.5	—103.6				
9 7	61.9	64.5	—112.7*		+21.8		+22.2
9 23	62.5	64.8	—117.8				
				— 14.0		—12.7	—15.9
9 39	63.2	64.7	—122.6				
9 55	63.4	64.8	—126.7*	.....	+ 9.1	.....	+ 6.3 ..... +15.2
10 11	63.7	64.7	—127.9				
				— 4.9		— 6.4	— 0.7
10 27	63.6	64.7	—129.4				
10 43	63.5	64.3	—131.6		+ 2.7		+ 5.6
10 79	63.8	64.8	—133.5				
				— 2.2		— 0.8	
11 15	63.5	64.7	—133.6				
12 10	63.6	64.0	—133.8*		+ 1.9		
12 45	63.6	63.7	—134.0				
				— 0.3			
1 01	63.6	63.7	—135.0				
1 17	63.6	63.7	—134.1*				

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EXHIBIT M.— *Continued.*

Experiment 10.

Change in length of G. in passing from 35.5° to 50.0°.
Removal to comparator at 7^h 10^m.

Time h m	T °	T °	Div.							
2 36	....	49.9	— 60.3*							
2 52	46.8	50.1	— 90.9	—50.1						
3 8	48.0	49.9	—110.4*		+26.5					
2 22	48.6	50.0	—124.8	—23.6		—11.3				
3 38	48.7	49.8	—134.0*		+15.2		+ 0.9			
3 56	49.0	49.9	—138.5	— 8.4.....		—12.4.....		+12.3		
4 10	49.1	49.7	—142.3*		+ 2.8		+13.2			
4 26	49.5	49.7	—148.3	— 5.6		+ 0.8				
4 42	49.7	49.9	—147.6*		+ 3.6					
4 58	49.9	50.0	—149.5	— 3.0						
5 14	50.1	50.1	—150.6*							

Experiment 11.

Change in length of G in passing from 86.0° to 63.0°.
Removal to comparator at 2^h 26^m.

h m	°	°								
2 37	66.0	64.6	—157.9*							
1 53	46.3	65.0	—152.8	+ 10.8						
3 9	65.4	64.9	—147.1*		— 3.4					
3 25	64.7	65.1	—144.5	+ 7.4		+ 0.3				
3 41	64.9	64.9	—139.7*		— 3.1		— 0.6			
3 57	....	....	.....	+ 4.3		— 0.3		+ 2.9		
4 13	64.2	64.4	+135.4*.....		— 3.4.....		+ 2.3.....		— 4.8	
4 28	....	....	.....	+ 0.9		+ 2.0		— 1.9		
4 45	64.8	65.1	—135.4*		— 1.4		+ 0.4			
5 1	....	....	.....	— 0.5		+ 2.4				
5 17	64.4	64.4	—135.0*		+ 1.0					
5 33	....	....	.....	+ 0.5						
6 50	63.0	62.3	—131.0*							

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EXHIBIT M.— *Continued.*

Experiment 12.

Change in length of G in passing from melting ice to 65.0°
Removal to comparator at $9^h\ 42^m$.

Time h m	Div.									
9 56	+ 93.89*									
10 12	148.95	+96.59								
10 28	190.48*		−56.19							
10 44	214.76	+40.40		+36.63						
11 0	230.88*		−19.56		−26.61					
11 16	241.90	+20.84		+10.02		+18.89				
11 32	251.72*	.....	− 9.54	.....	− 7.72	.....	−8.86			
11 48	259.30	+11.30		+ 2.30		+10.03				
12 4	263.02*		− 7.24		+ 2.31					
12 29	264.78	+ 4.06		+ 4.61						
12 36	267.08*		− 2.63							
12 52	268.32	+ 1.43								
1 8	268.51*									

OMISSION. — Insert the following after division (17), page 74:

The standards compared must remain in the same position with respect to each other, and with respect to the base upon which they rest throughout the entire series of observations undertaken.

If the adjacent vertical faces of the standards are in actual contact, the amount by which the change in length fails to follow the indications of the thermometers will not be the same as when the standards are separated by an interval of e. g. one centimeter. In other words, the character of the systematic relations between the residuals will undergo a change in passing from contact to independent positions. One bar seems to hold the other in check during variations of temperature. In a general way the surface indications seem to give more correct results when the bars are in contact, and the indications of the thermometer at the center of the standard bar seem to be more reliable when the standards are placed upon blocks of wood and separated one or two centimeters.

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A CONTRIBUTION TO THE LIFE HISTORY OF THE DIATOMACEAE — PART II.

H. L. SMITH, Hon. F. R. M. S., Geneva, N. Y.

III. *Motion of the Diatoms.*

Various explanations have been offered to account for the movements of the Diatoms. Ehrenberg supposed that he had observed a sort of protuded foot, others have imagined they could perceive ciliæ, and some very good observers think they have seen an undulating protoplasmic sheath outside the frustule. None of these explanations are really satisfactory, and we may affirm without much fear of contradiction, that, up to the present time, the whole subject is involved in obscurity.

Without attempting a review, or critcism of the work of others, I shall confine myself here, simply to a statement of my own observations bearing upon this subject, presenting them for what they may be worth, and regretting that, upon the whole, they are not more satisfactory. In investigating the cause of the movements, it may be remarked, that in a general way, all diatoms having a raphé, like the Naviculæ for example, when they are moving, present the valve and raphé to some solid substance, gliding along, e. g., on the surface of the glass slide, or the covering glass, when we examine them with the microscope, and if by a slight movement, or some disturbance, they are rolled over, they quickly right themselves and are again presented in "side view" with the raphe towards the surface of the glass, or may be, rise up on one end and swing around. This fact is familiar to all observers of living diatoms, and because of this peculiar-

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ity, they present themselves always, or almost always when moving, as I have said, in "side view," i. e. with the valve, not the sutural zone, towards the eye. The diatoms without a raphé, as the Nitzschiæ, which have very lively motions, and the Surirellæ which move more sluggishly, glide along with the ala, or keel, in contact with some foreign substance, or, if they themselves are not moving, little particles of matter can be seen running along the ala, or the keel; or the raphé, if the diatom belongs to group I. This movement of small particles in a contrary direction to that in which the diatom was moving, before it was stopped, can be very readily observed, and is too well established to be questioned. When one has watched a diatom for sometime, moving forward in a certain direction, and then perceives the path suddenly to be retraced, this appears to be so much like a voluntary action, or as if in search of food, that we do not wonder that many of the earlier observers, including Ehrenberg, regarded them as animals. Most of my own experiments have been made with large specimens of *Navicula* (*Pinnularia*) *major*, and it was in November, 1865, that I first detected, in observing this diatom, the very curious phenomenon that I shall now describe. Several times since, I have seen the same, but never more perfectly than when first discovered. I had made a pretty deeply colored solution of the ordinary water color pigment, indigo, which, while showing to the eye a dark blue tint, exhibited under the microscope numerous minute particles undissolved, and in fact, it was but the myriads of these particles out of focus, that gave the general tint to the field. I transferred to the indigo field some specimens of large living *Navicula major*, which were in a healthy condition and had been recently gathered; I perceived immediately something that gave the impression of a transparent slime, or mucus sheath, outside these diatoms; that this appearance was due to some transparent investing mucus or jelly-like substance, and not an optical illusion, was manifest from the gathering together of the particles, in a lunate manner in

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front of the moving diatom, as they were pushed forward and by their opening behind. This appearance is represented around the smaller of the two diatoms figured in Plate I, and I could also see as represented in the plate, the little undissolved particles, running along the median line, or raphé, towards the central nodule, here they stopped for awhile and then finally passed out focus. Although the gathered particles, pushed in front of the moving diatom, seemed to indicate beyond a doubt the presence of an external transparent sheath, thinner perhaps along the median line or perhaps perforated, I was not satisfied, and hastened to examine other diatoms, I soon found that there was a very great difference in the amount of this external slime, some diatoms showing it abundantly, and others very sparingly, and indeed, the same diatoms, sometimes exhibited more and sometimes less of it; and as a general rule, those with the darkest endochrome showed the least of this external substance. To test still further whether the appearance was an illusion, I put large frustules of the prepared diatoms, and also some dead forms, but still having more or less endochrome in them in a similar field; these presented no such appearance as that I had observed with the living specimens, though in the case of some of the dead forms, still having the endochrome in them, I could perceive traces of it. So strongly apparent is this investing sheath, that it can be seen excellently with as low a power as a two-thirds inch objective, if the diatoms are large and in a healthy condition. To remove all doubt, I put into the same field, two large specimens of the same diatom, and of equal size, one quite active, the other dead; the investing substance was quite evident around the former, but not around the latter. Again, I warmed a slide with the Naviculæ in the indigo field, but not so as to be uncomfortably hot to the hand; the diatoms were killed and the endochrome changed in color to green, the frustules appeared now to be inside of almost colorless sacs surrounded by the indigo, and tinted only by the particles of indigo out of focus above and

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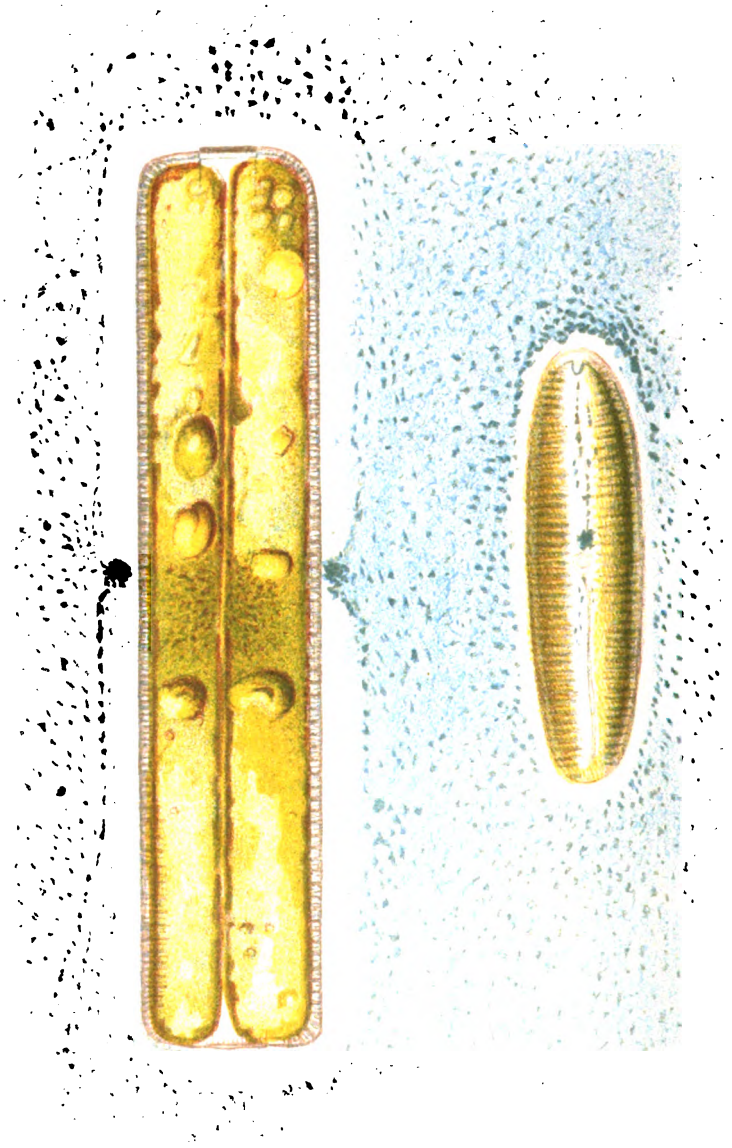
PLATE I



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below them. I rolled the frustules over and over repeatedly and across the field; the investing substance, whatever it was appeared to be something insoluble in water, though, as I found out afterwards, and as I have already mentioned in Part I. it is readily tinted by fuchsine, and, in fact, it seems to be, but in much smaller quantity, the same substance that is found around diatoms in the act of conjugating, and which is in this case, readily recognized as it is more or less stained or dirty, as well as more abundant. A large specimen of Stauroneis, did not exhibit a trace of this mucus. But again it was quite distinct surrounding many desmids, (e. g. Euastrum), and when these latter were moved about they left a trail behind.

Not only do the little particles glide along the raphé of a Navicula, as I have described, but there is something more wonderful, and it was quite startling to me when I first observed it, as I now seemed to be getting some clue to the cause of the movements of the diatom. I have endeavored to represent the phenomenon to which I allude in the larger figure, Plate I., and the diatom is here presented in front view; it had been stopped in its movement by some foreign body, not shown in the drawing.

I saw the little particles of indigo, streaming along on either side, by a series of jerks, towards the central nodule. When they arrived at the end of the raphé, just where, on the prepared valves of the large Navicula, a distinct point, or dot, can be seen at the end of the raphé, near the central nodule, they stopped, and were here gathered into *rotating balls*, which, after acquiring a certain size, suddenly broke, and the individual particles streamed off as shown in the drawing in a direction opposite to that in which the frustule had been moving before it was stopped. I noticed this curious phenomenon for hours, and saw ball after ball thus formed and dispersed, and have since shown it to others. Sometimes, the motion would stop for an instant, and then commence suddenly in a reversed direction, the particles now running

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along the raphé on the other half of the valves and the diatom itself commenced to move in a reversed direction. In other words, when the diatom tended to move in any direction, the particles would move in the opposite one; but when the diatom was moving freely, the little particles were quiet, it was only when it was stopped, or delayed by some obstacle, or about reversing its motion that the particles began to move, and form the balls that I have mentioned. In plate I., the particles are shown some distance from the valves, but I have, in other cases, noticed them almost or quite in contact, and, in any case, it would seem as though there must be an opening or slit in the mucus sheath, or perhaps the elevated ridge of the raphé, or ala, extends through it, and it would appear also that a true hole or opening in the valve existed at the "dot," at the end of the raphé, near the central nodule, and that here at least, there was more or less of an opening through the investing sheath. I have frequently observed (in front view) a difference in thickness of the sheath on the two valves, and sometimes a movement of the particles on one side, and not on the other, in which case the diatom turns around or even lifts itself up. The rotation of the little balls of indigo, which is the most curious part of these observations, there can be no doubt about, and we may well ask, how is it to be explained? One can readily fancy a minute jet of the fluid, issuing from the interior of the frustules, through the little opening, or dot, and thus causing this rotation, it is not unlike the well known hydraulic experiment which I have frequently exhibited of a cork ring clinging to the side of a jet of water and rotating; but if this be so, where is the water entering the frustule to keep up the supply? Is it possible that, on the "central band," already mentioned in Part I., as a fulcrum, the valves of the frustules alternately open and close at either end, thus causing an ingestion on one side, and an ejection at the same time of the previously ingested fluid at the other? And, if this be so, what is the cause of this motion, from which, if it exists, that of the diatom itself may result?

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It is not difficult to conceive that if osmotic action is going on, and one can not doubt that this is the case, a difference of tension may exist in the two halves of the frustule, on either side of the middle band; and this would produce a bellows-like movement of the valves, and give rise to the issuing jets on the one or the other side. I set myself therefore to work to determine, if possible, whether any such motion of the valves, as here supposed, does in reality exist. The observations were made with difficulty, as the diatom must not only be large and quite lively, but it must be arrested in its movements if micrometrical measurements are to be made, and yet, itself, so far as the valves are concerned, exhibit the movements, if any really exist, which, under ordinary circumstances, would have resulted in motion of the diatom.

Selecting a large specimen of *Navicula major* which was imbedded so firmly in a slime mass that it did not move, and which was presented in front view, I carefully adjusted the spider lines of an excellent Powell & Leland micrometer on the over-lapping edges of the hoops, or zones, I watched this specimen for half an hour, and observed, during this time, a movement of increase and decrease, and the whole extent of the variation in breadth, in the opening and the shutting of the edges of the hoops, and presumably the movement of the valves, amounted to between 10 and 15 divisions of the micrometer head, or, with the power I was using, 1 50.000th of an inch. Subsequently I repeated the experiment on a very large *Stauroneis Baileyi*, one end of which is shown in Fig. 1.

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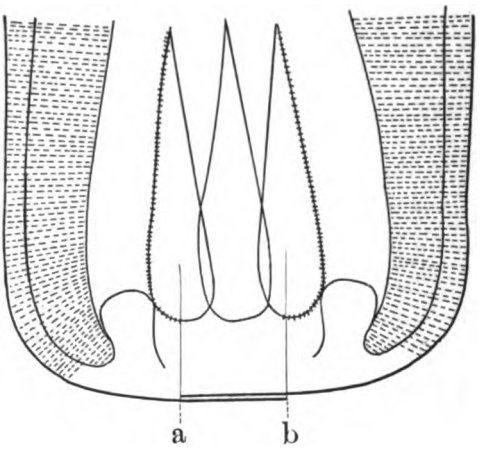

Fig. 1.

This diatom was partly free, and could occasionally move a short distance among the débris in which it was imprisoned, and I was enabled to make tolerably satisfactory measurements. When the movement of the frustule was in the direction of the arrow I found, in one case, a change of distance occurred between *a* and *b* equal to 19 divisions of the micrometer head, and shortly after, an increase of 4 divisions more, and I satisfied myself that the frustule opened slightly in the forward moving end; I am unable to say whether, at the same time, it closed an equal amount at the other end. I need not remark that, at first, this was just the opposite of what I had expected; for I had expected to find that, in moving forward, the anterior portion of the frustule would close, inasmuch as the issuing jet which had caused the rotation of the indigo balls came from this portion; the explanation of this anomaly is not difficult. I have already mentioned that the internal contents of the diatoms are enclosed in a membraneous sac, which is, no doubt, elastic, and at all times under tension. If we suppose endosmose in one half, and exosmose in the other, there would be a swelling of the sac enclosing the internal contents on one

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side and a diminution on the other, attended by an opening of the valves in the swelling half; this would be attended also with an expulsion of the water lying between the membrane and the under surface of the siliceous valve on the same side, and a reception of it on the other — though probably, now not along the raphé and through the dot, but in the opening line of suture, as the particles of indigo are never to be observed moving along the raphé of the following end. Doubtless the water containing the carbonic acid, and whatever else there may be in solution capable of being assimilated, bathes the membraneous sac in between it and the inner surface of the siliceous valves, and the nutritive material finds entrance into the sac by endosmose, alternately at the opposite halves, the expelled water being that from which the nutriment has been taken, more or less completely. I am disposed, therefore, to consider that the motion of the Naviculæ is due to injection and expulsion of water, and that these currents are caused by different tension of the membraneous sac in the two halves of the frustule. In those diatoms which do not have the central band thus binding the frustules, as, for example, the Synedræ, the Fragilariæ, and the circular and angular forms, no motion is to be observed, or at best but a slight trembling, as, in this case, the tension is more nearly uniform over the whole surface of the internal sac.

In the representation of one end of Stauroneis, given in Fig. 1, markings may be seen on the internal zone, corresponding to the large thickened infolding of the valves, known as *vittæ* in the smaller forms, just within where the internal zone is connected with the valve. These markings indicate successive points of growth of the internal zone, or connecting membrane, which increases in breadth, probably not by a deposition of silex on the outside edges, but at the junction with the valves, and the smaller amount of silex here, or the less perfectly siliceous character compared with the other portions of the frustule, is indicated by the slight adherence

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of the zones to the valves, in all except the filamentous species, noticeable especially after treatment, and also by these zones remaining often projecting over the new frustule, after self-division, as in Melosira, and Biddulphia. To return to the movement of the indigo particles, one can observe this to occur with great rapidity along the ala of the Surirellæ and the keel of the Nitzschiæ, and the motions of the latter are especially rapid. In these two genera the particles run along the whole length of the ala or keel, but generally only on one side, though I have seen them in the case of Surirella moving up one side and down the other at the same time, producing now the "languid roll." So far as the Nitzschiæ are concerned, and which have the most lively motions of any diatoms, the structure appears to be that of a Navicula without central nodule, and so, having a complete (not divided) raphé, which runs now, not somewhat near the middle of the valve as in the Naviculæ, but along one edge, directly in contact with the sutural zone, it is along this that the little particles can be seen to move with great rapidity when a large Nitzschia has its motion stopped by meeting some opposing obstacle, but I have never witnessed the formation of the balls, as in the case of *Navicula major*, nor have I ever detected any of these movements in the frustules of Group III. As for the presence of ciliæ, I have, hitherto, been unable to detect them, using various methods of illumination, and the very best objectives; and I have, for this purpose, watched for hours the changes in the endochrome, which are sometimes quite pronounced, but always with the same negative result. I had hoped to be able to detect something by the aid of *intermittent light*, and I used for this purpose, the bright flash of a powerful induction coil, condensed by Leyden jars. I found it somewhat difficult to manage, but it gave very sharp views of the ciliæ of rotifers, which, although they were in rapid motion, appeared to be still, but I could do nothing with the diatoms. The experiments however, were crude, and I have not the least doubt

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that very fine and close lines can be seen with this mode of illumination more readily than by a continuous one, but the source of light should be very brilliant to give sufficient illumination. I have remarked that, as a general rule, the Nitzschiæ are the most active diatoms, though some of the Naviculæ e. g. *N. acuta* are not far behind them. And, though it seems to be necessary as a general rule, that the raphé, or the keel, should be in contact with some foreign substance, in order to produce motion, it is not always necessary. Thus, if one puts into a saucer, partly filled with water, some material containing diatoms, and exposes it for a few days to the light, and especially if protected from the dust by a sheet of clean glass, a thin pellicle or skin will be found all over the surface of the water, charged with diatoms, and later, a slime skin will be found on the bottom and sides of the vessel, also full of diatoms; and often, many species of different genera mixed. This "*thallus*," as it has been called, is a favorable "nidus" for many germs of the minute algæ, so that, along with the diatoms, one will find myriads of round, green or olive tinted spores, which have sometimes been supposed to be a phase of diatom growth. On damp walls, and where the water is dripping from an aqueduct, and around the overflow of basins of fountains, I have found these mucus skins in abundance, and in them, a great variety of forms associated; and not unfrequently, in quiet waters, these skins, stripped off from the stones or submerged plants by the buoyant power of the gasses eliminated in the sunshine from the diatoms and minute vegetable organisms, may be observed floating on the surface. A somewhat similar thin slime skin, appears to be left by some gasteropods, after moving about for some time, shell downwards, on the surface of the water. I made a curious experiment with *Physa heterostropha*, which after thus moving about awhile on the surface of the water, now began slowly to descend — so gently, and slowly, that it appeared to be suspended by an invisible thread attached by one end to the slime skin and

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which it was now paying out, as it were, in its easy descent; with a pair scissors, carefully introduced into the water, I snipped this invisible thread, and instantly, the snail dropped like a shot to the bottom, I have often repeated this experiment which was performed in the first instance at the suggestion of one of my colleagues, and is really his discovery. Now, is there any such thing in the case of the diatoms? I I have not been able quite to convince myself of this, and yet, they often retrace their motions in a path very suspiciously near to that pursued before, as though guided by some Ariadne's thread, and I have often seen a diatom moving and tugging behind it, as if by such a thread, some small object, by a series of jerks, or twitches. I am sorry that these experiments, to determine if possible some cause for the movements of the diatoms are so meagre, and that I can offer so little towards a satisfactory explanation. It is nearly twenty years now since they were made, and we must confess that that our knowledge is still very imperfect, very little if any, in advance of what was known then.

IV. *Reproduction and Growth of the Diatoms.*

Here, as well as concerning the motions of the diatoms, there are many points still quite obscure, and much of what has been published, even recently, is based upon the statements made many years ago in the "Synopsis of the British Diatomaceæ," and which are decidedly untenable, and very far indeed from being proved. It was assumed by the Rev. W. Smith, in the work just named, that the different genera may be grouped, as to their reproduction, into four classes, and this assertion has been copied by nearly everyone who has written upon the subject. It is erroneous, inasmuch as we cannot select any one genus as a representative of this or that class; and as for the classes themselves, I am pretty certain that the fourth one which he mentions, has no existence whatever.

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Conjugation, the act of reproduction, occurs in one of three modes.

(1) Two frustules unite their undifferentiated endochrome (true conjugation) — one a sperm cell, and one a germ cell, and thus, *one sporangium* is produced from *two parent* frustules.

(2) Two frustules, with differentiated endochrome in each, unite, and produce *two* sporangial frustules — two sperm cells, and two germ cells — that is, a sperm cell and a germ cell in each of the parent frustules, these reciprocally act upon each other, and the germ cell of one, fertilized by the sperm cell of the other, gives rise thus to *two sporangial cells* from *two parent* frustules.

(3) The differentiated internal contents of a single frustule reunite, producing *one sporangium* from *one parent* frustule.

It is difficult to see how this latter is accomplished, nothing like a sexual character has yet been observed, but after all, it is not unlike the same stalk bearing in the compass of the same flower, the ovule and the fertilizing pollen. How the Case IV. of W. Smith, as stated in the British Diatomaceæ, can occur, I am at a loss to concieve; he supposes a *single parent frustule* to produce *two sporangial frustules*, and cites in proof Achnanthes, and Rhabdonema. Now, I have witnessed the conjugation of *A. subsessilis* and of *A. exilis*, and in each case there were evident enough, two parent frustules. I can only account for the statement in the Synopsis by supposing that only a few specimens were observed, and these imperfect, and that one of the parent frustules was accidentally removed. As for Rhabdonema, the figure given in the British Diatomaceæ is by no means conclusive. In all the filamentous forms that I have observed conjugating, e. g., Melosira, and Orthosira (as figured also in the Synopsis), as also in Odontidium and Fragilaria, the case is that of a *single parent* frustule producing a single sporangium. In plate E. Fig. 1, of the Synopsis there is

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shown plainly enough the two differentiated portions of one parent frustule passing out from the halves of the same within the enclosing gelatinous sac; and again, these are gathered into two more condensed portions in Fig. 2, and two sporangial frustules are apparently shown in Fig. 3 of the same plate though I think they are but the valves of one sporangium after self-division. Dr. Pfitzer, in the work I have already alluded to,* enumerates some sixty species, belonging to twenty-six genera, in which conjugation had been observed so far as he was informed up to 1871. Thirteen of these, belonging to nine genera, I have myself observed, and to his list I can add fifteen species, and four genera; they are named below, where the first column shows the class, and the last the date of observation.

Class.	
2	*Gomphonema dichotomum*, Oct. 1861.
3	" *tenellum*, Feb. '59, Mar. '60, Mar. '63, Feb. '64, Mar. '66, Feb. '68.
2	" *olivaceum*, Mar. '60, Dec. '62, Nov. '64, Apr. '79.
2	" *ovatum*, n. sp., Feb. '59, Mar. '60.
2	*Cocconema parvum*, Nov. '60, Oct. '62.
2	" *cymbiforme*, Nov. '60, June '60, Feb. '63, Aug. '65, Dec. '65, Oct. '72.
3	*Cymbella cuspidata*, Mar. '62.
2	" *Helvetica*, June '60.
2	" *affinis*, Oct. '62.
2	*Achnanthes subsessilis*, Aug. '60.
3	" *exilis*, Oct. '65.
2	*Cocconeis pediculus*, Oct. '66, Oct. '69.
2	" *placentula*, Oct. '69.
2	*Navicula amphirhynchus*, Mar. '62, Mar. '66.
2	" *atomus*, Dec. '59.
2	" *Viridis*, Mar. '62.

* Ueb. Ban u. Entwickl. d. Bacillariaceen, Bonn 1871.

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2 *Stauroneis gracilis*, Mar. '62.
3 *Nitzschia linearis*, Nov. '65.
1 *Suriella splendida*, Feb. '59, Oct. '59, July '60.
3 *Meridion circulare*,, Mar. and Apr. '68.
3 " *constrictum*, Mar. and Apr. '68.
3 *Fragilaria capucina*, Mar. '61, July '64.
3 *Odontidium hyemale*, Dec. '59.
3 *Orthosira punctata*, July and Aug. '65.
3 *Melosira Borrerii*, Apr. '62.
3 " *varians*, Feb. '59, Oct. '59, Sept. '65, Apr. '68.
3 " *nummuloides*, Apr. '62.
3 *Synedra ulna*, Oct. '66.

Thirteen of these belong to Class 3, one parent frustule producing one Sporangium; fourteen to Class 2, two parent frustules and a double sporangium; and only one to Class 1, where two parent frustules form one sporangium, and not a single instance of W. Smith's Class IV. It is not a little singular that the instance he gives of his Class I. (my Class 3), is *Himantidium pectinale*, which is contrary to all other experience with the filamentous forms, and indeed as conjugation occurs *in the filament*, where the *valves* of separate frustules lie opposed, while for conjugation, it is necessary for the *connecting zones* to be in this position, one is at a loss to understand how his fourth method of conjugation could be effected in this species, while it was yet in the filamentous condition. So far as the figure in the S. B. D. illustrating the conjugation of Himantidum is concerned, it shows no evidence at all of the two parent frustules, but rather of one whose detached valves can be seen at either end of the sporangium, and which is shown *double* in one of his figures; but here, as with the Rhabdonema, from self-division. In the majority of cases which come under Class 2, the parent frustules are imbedded in a mass of jelly, which, at this time appears to be secreted in great abundance; they

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lie side by side for some days, the connecting membranes, or zones, in contact, not the valves; certainly three days in the case of *Gomphonema olivaceum*, and, during this time the internal substance swells, and the frustules open at the sutural line, and the two halves of the frustules are soon pushed so far apart, that the membranes enclosing the internal substance of the two frustules come in contact, and absorption taking place, the two endochromes are united, and segregation commences; two ball-like portions are formed, which are the rudiments of the future sporangial frustules, and as these are developed, they carry with them the valves of the parent frustules still adhering to the outer membrane which encloses the new individuals. Dr. Lanzi, of Rome, if I understand his meaning, seems to have made the mistake of a frustule containing germs, changing into a sporangial cell, and speaks of a series of tranformations, until the "Thallus," as he calls it, is changed into germs and frustules in various stages of development, which, if true, would be an entirely different mode of reproduction from anything I have ever observed. I am quite certain, from the many times I have seen *Gomphonema olivaceum*, and other diatoms conjugating, imbedded in mucus masses, which were largely a tangled mass of stipes, and charged with germs, not of diatoms, but of various algæ, that the "spores of diatoms," and supposed "diatoms in various phases of growth" seen by him, were not really such, nor indeed in any manner a phase of diatom growth, and I think I may say that there is no other mode of reproduction other than the formation of a sporangial frustule in the manner already described. This process as first noticed by Thwaites, and since by scores of others, is fully adequate for the purpose of perpetuating species, it is quite clear and simple, and in entire harmony with all the well-established phenomena of plant reproduction; but I shall have something more to say of "sporangial spores," and "brood diatoms," and their supposed development from minute germs, hereafter.

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In the conjugation of some species, *e. g. Navicula viridis*, and *Stauroneis gracilis*, a rugose siliceous sheath is formed before the valves of the sporangial frustule make their appearance; this is generally cylindrical, with cap-like ends, and its rugosity appears to be due to its increasing along with the growing sporangium. This sheath has been by some considered as the sporangium, and the frustule formed within it has been improperly called the "Auxospore;" but it is not always present, and in fact is only found in a few genera. Representatives of this sheath may be seen in plate IV., and I shall allude to it again. Previous to conjugation, which occurs most frequently in the spring, the endochrome of the frustules preparing to conjugate, becomes paler, the colorless portion more abundant, and innumerable minute dark particles may be seen in incessant trembling motion, not unlike the granules in the little cavity at the ends of a lively Closterium; this phenomenon is particularly noticeable in the Gomphonemæ. I have tried in vain to discover some perceptible distinction or difference between the conjugating individuals. I fancied at one time that the little particles might be instrumental in effecting fertilization, but I have since observed them in great abundance in frustules where, evidently, the endochrome was already in a state of decomposition; of course I cannot positively assert that these latter were identical with those observed in the conjugating particles, they appeared to be the same. The amount of mucus, and its consistency, varies with different species; with the Gomphonemæ and the Naviculæ, there is considerable; with Stauroneis, very little. Again, it is very tough and strong with Surirella, and none at all with Melosira. These conditions may not obtain universally, and, doubtless, they are modified to suit circumstances.

The time required for conjugation varies from six to eighteen days, and I give below the actually observed and measured growth of the sporangium of *Cocconema cymbiforme*, which I found conjugating Oct. 27, 1872.

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Length of sporangial frustule at commencement,		.000833"
" " "	in 24 hours,	.000942"
" " "	in 36 "	.000978"
" " "	in 48 "	.001051"
" " "	in 60 "	.001123"
" " "	in 72 "	.001305"
" " "	in 84 "	.001522"
" " "	in 96 "	.001631"
" " "	in 108 "	.001885"
" " "	in 120 "	.002030"
" " "	in 132 "	.002175"
" " "	in 144 "	.002465"
Total 6 days: increase first	day,	.000109"
" second	"	.000109"
" third	"	.000254"
" fourth	"	.000326"
" fifth	"	.000399"
" sixth	"	.000435"

The rate of increase was much more rapid as the sporangium approached maturity. I kept these specimens, and numerous others to be mentioned in the record of measurements during conjugation, self-division, etc., living in an entirely healthy condition by means of the "growing slide," which I contrived for this purpose, and described in "Silliman's Journal" September, 1865.

The time observed for completion of conjugation of *Gomphonema olivaceum* observed in December, 1862, was nine days. The sporangium, when first formed and gathered into balls, measured .0005", and when fully grown .0018", and a sporangial frustule that did not exhibit a trace of division line at 9 A. M. exhibited at 11 A. M., next day, a very strong one. The Gomphonemæ generally, and the Naviculæ belong to Class 2. Two parent frustules and two sporangia, and frequently, as already noticed by Mr. Carter, there is a difference in the size of the former, but not always, and this is

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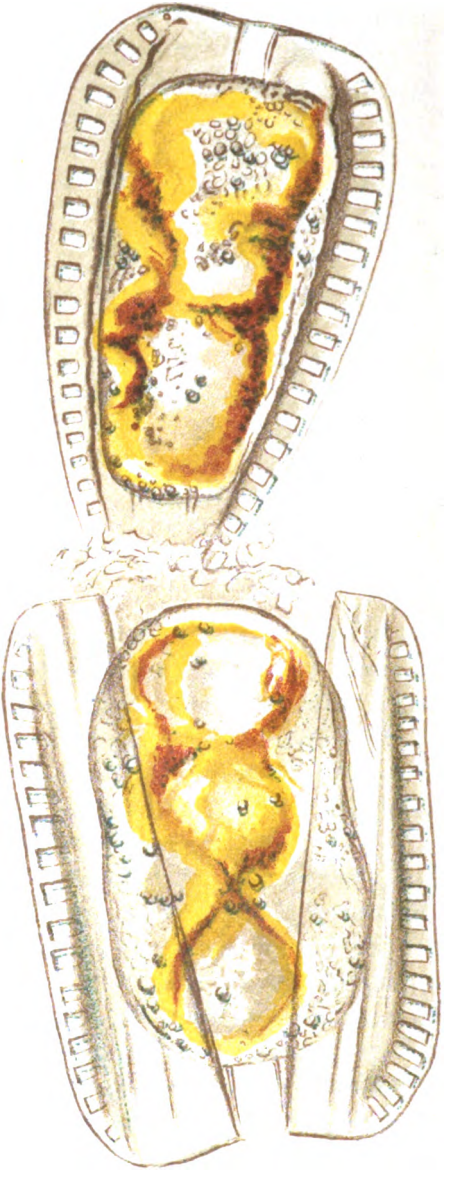
PLATE II



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probably a character of no significance. Among the Gomphonemæ there is one form which I consider as *G. tenellum*, that I found for several years conjugating, and only one parent frustule producing a single sporangium (Class 3). All others of this genus, so far as I am informed, belong to Class 2.

It will give a somewhat better idea of the conjugation of the diatoms to review particularly one or more instances belonging to each of these classes.

Class I. Two parent frustules and one sporangium, as in *Surirella splendida*. The conjugation of this fine diatom was, I believe, first noticed by Focke, but his specimens were not numerous enough, nor did he succeed in keeping them living long enough to arrive at accurate conclusions. His figure was copied in the Micrographic Dictionary. I found numerous examples of this diatom in the act of reproduction in October, 1859, and again in July, 1860. The two parent frustules unite at the smaller end, and in about two days, the sac containing the internal contents becomes so much swollen, as to open the frustule at the sutural line, and the vitalized enclosing membranes are thus brought into contact. I have represented this phase in Plate II. One of the parent frustules has already opened, and the enclosed sacs are well marked. As soon as the membraneous walls become obsolete at the place of contact, fusion of the internal contents begins and is completed in about six hours, at the end of which time the whole of the colored portion of the two individuals is gathered into a ball, and the colorless portion much resembles the white of an egg surrounding the yolk. This phase of growth I have represented in Plate III., and I have copied the appearance of the endochrome in both plates as faithfully as I could. I have never observed any further subdivisions and segregations of the yolk mass, or colored endochrome. At this early stage the commencement of the central band can be discerned, and up to this point, in the enclosing sacs (see Plate II.) and in the round

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ball (Plate III.), the colorless portions were full of small globules, of not very much higher refractive power than the plasm in which they were embedded. I cannot here give a representation of the nearly completed sporangial frustule, which, if given on the same scale (magnified about 400 diameters), would be too large for one plate. It is about double the size of the parent frustules, and the swollen mucous envelope, seen around the ball in Plate II., is correspondingly enlarged, and still has the empty halves of the parent frustules adhering to it. The enclosing jelly-like or mucous sac, is not vitalized, it is simply an exuded substance, not entitled to be called the "sporangium" in the sense in which this name was given by Mr. Thwaites; but the continental authors have sometimes called it so, and have given the name "auxospore" to the sporangium of Thwaites. As the sporangial frustule of Surirella approaches full growth, the canaliculi begin to show well developed, the endochome is however pale, and a very large portion of the newly formed frustule is occupied by the colorless substance with the little balls. The final result of the conjugation is a large frustule, with much more rounded ends than the parent frustules, and valves not so wedge-shaped; it is, in fact, the *Surirella nobilis* of W. Smith, well shown in the figure given in the synopsis of the British Diatomaceæ. It exhibits very plainly the two nucleii of *Surirella splendida*, as described in Part I., as it should, being really one of the phases of growth of that diatom, and the specific name of W. Smith given to the sporangium must be deleted. I am not able to state the period required for completion of the conjugation, as I had not, at the time I found the diatom in this condition, invented the growing slide, and I could not, therefore, keep them living healthy and long enough to make as complete observations as I was able to do subsequently with other diatoms.

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PLATE III

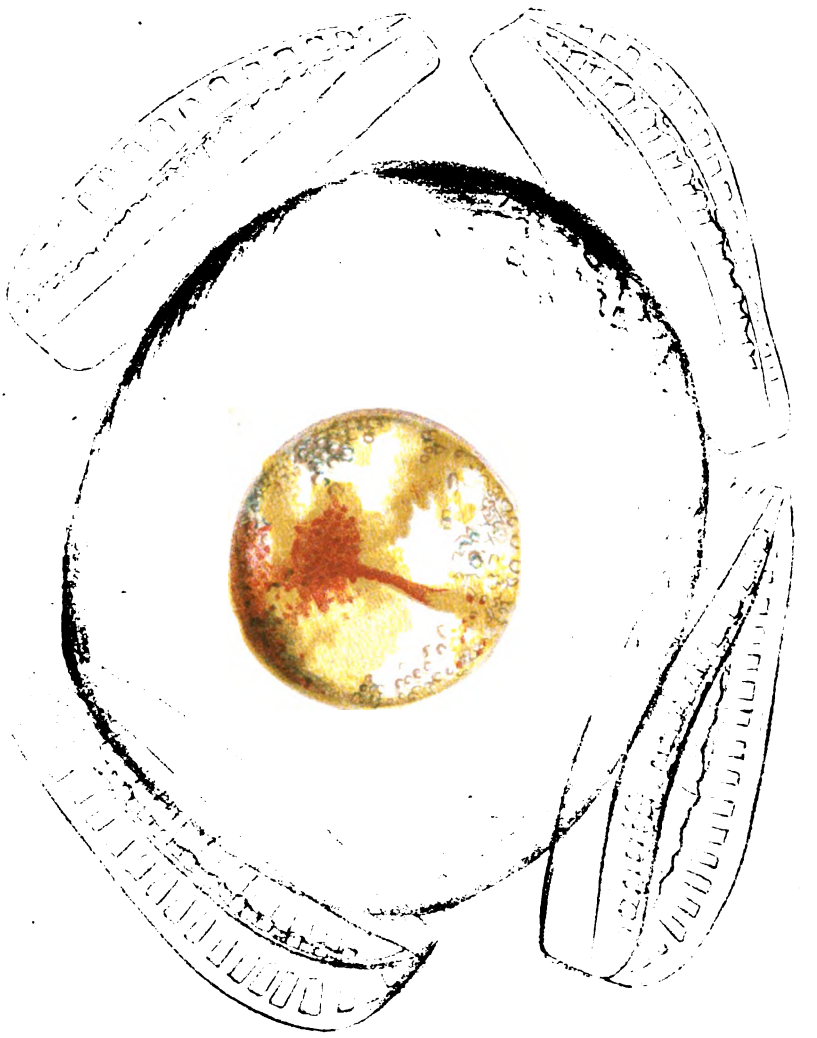

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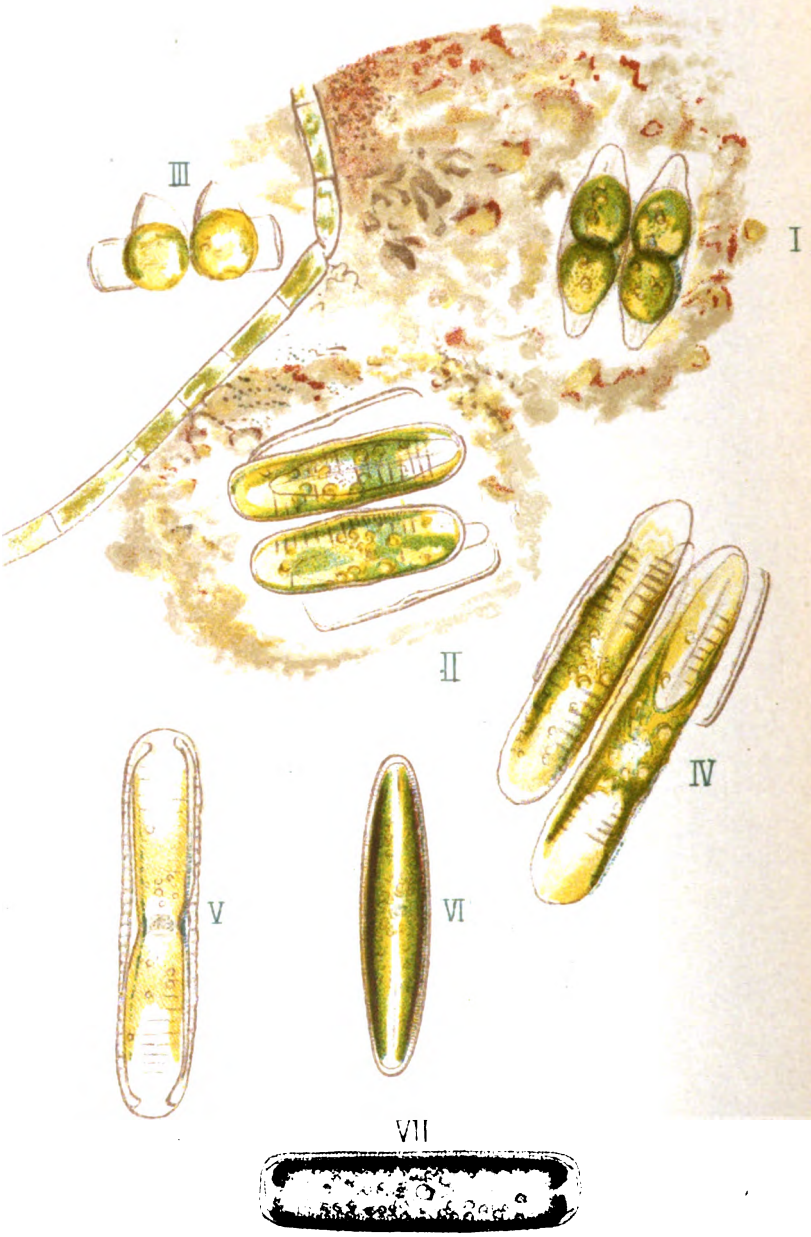
PLATE IV



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CLASS II. Two parent frustules and two sporangial ones.

I select as the first example of this, *Navicula amphirhynchus* (*affinis*) which I obtained conjugating in March, 1862, and again in the same month, 1866. The parent frustules lie side by side Plate IV., Fig. 1, the endochrome soon becoming segregated into two well defined portions in each, and at this time, numerous minute dark particles, as already mentioned, may be observed trembling all over the endochrome sac in a very lively manner—these I have partly represented. Somewhat later, the double endochromes unite, forming two sporangial frustules (Fig. 2), which now grow side by side, and seen endwise, they appear as in Fig. 3. It would appear that the segregated portions (germinal and seminal?), though combined in the same individual and not in separate individuals as in Class I, act mutually upon each other, and so produce a double sporangium instead of the one just mentioned; and that in fact, it is a step removed in the scale of development from Class I. At a later date a rugose sheath is formedaround each sporangium, and at this time the jelly-like mass, that before enclosed the frustules, and which always has more or less of rubbish, grains of sand, &c., adhering outside, so that one has to focus through all this to get a view of the frustules themselves, disappears; and perhaps, this rugose sheath comes from the contraction and shrinking of this same silicious material. This phase of growth is represented in Fig. 4; the valves of the parent frustules may be seen still adherent, as the sporangial frustules move about. Fig. 5 shows the more matured sporangial frustule and rugose sheath in front view, and the completed form after the frustule slips out from the rugose sheath is shown in Figs. 6 and 7, in side and front view, and is what has been called *Navicula firma*. In some cases one of the sporangial frustules has proved to be abortive, but this did not appear to effect the growth of the other. I was able to make long continued and accurate observations of

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this diatom, and to measure the growth of the sporangium from day to day. The average time required for the conjugation, was fourteen days, the length of the newly formed sporangium was .0018" and when completed .00467"; the rate of growth was quite uniform. In the same gathering I found some of the sporangial forms conjugating, and so producing abnormally large forms, and I have witnessed the rupturing of the rugose sheath of one of these large individuals, and have seen the frustule escaping from it, leaving the sheath behind empty. I regret that, under the circumstances I cannot give the representations of *Navicula viridis*, and *Stauroneis gracilis* conjugating, these both, belong to Class II., and in both, I have observed, one sporangium to be abortive, and so apparently, if one should observe it near the completion, it might appear as coming under Class I. The rugose sheath of *N. viridis* is very firm, and it is not unfrequent in mounts of the prepared frustules of this species. *Stauroneis gracilis* is an interesting example, belonging to Class II. I obtained this diatom in the early spring of 1862, along with a mass of floating confervæ, from a small pond well warmed by the sun. The investing gelatinous sheath, was very tender; the slightest pressure on the cover-glass, would scatter the valves of the parent frustules, and the sporangium, often as a perfectly round ball, would roll about in the field. Unlike *Navicula amphirhynchus* and *N. viridis*, which, when even but half developed, are strongly enough impregnated with silex, to withstand strong heating, and of which I prepared many beautiful mounted specimens, those of *Stauroneis* are so tender, that I was unable to obtain any specimen which, after the burning, would show the rugose sheath. This sheath is however present but it is much smoother than that of the diatoms just named. The globular shape of the young sporangium, is soon changed into an elongated barrel-shaped one, circular when seen in end view. Very soon after it becomes elongated, the endochrome shows a definite arrangement, and when viewed

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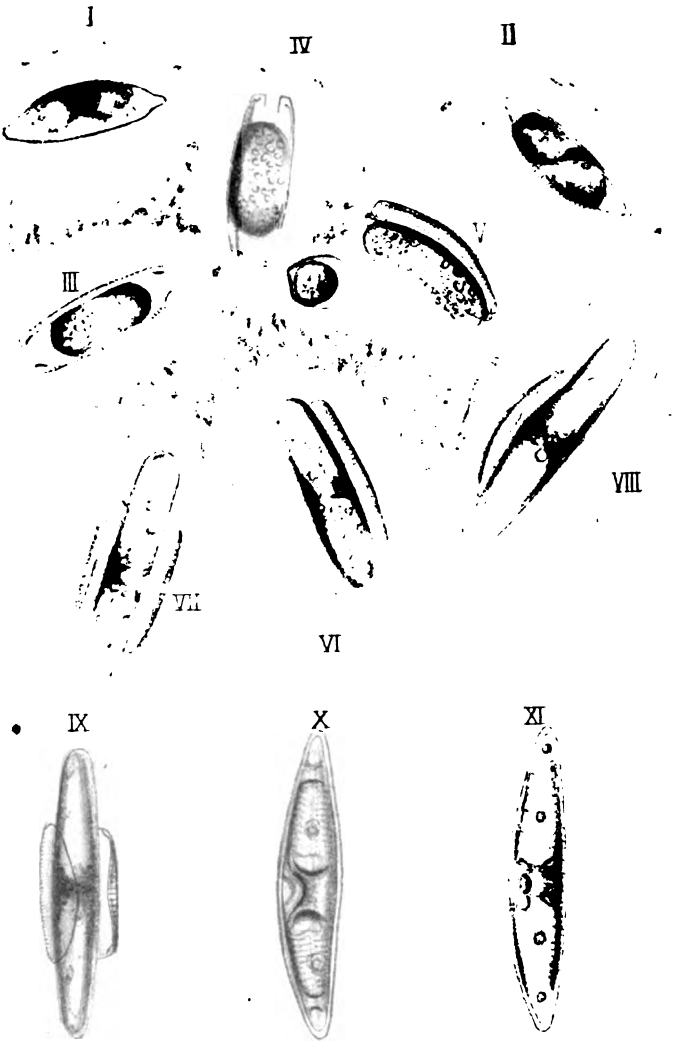
PLATE V



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endwise, a perceptible thickening of the walls may be perceived, corresponding to the enlarged central nodule of this species. The sheath has a cap on each end, and I have frequently seen the frustules swimming about with these caps still attached to their ends, while the central part of the sheath was gone. Mr. Carter has figured the same for *Nav. serians*, but I have never seen it break up into the thread-like phase which he describes, nor indeed can I see how this could happen. The time required for the conjugation of *Stauroneis gracilis* is from eighteen to twenty days, and the result, or final development of the sporangial frustule is *S. phoenicenteron* of W. Smith and well figured in the "British Diatomaceæ," this specicific name, however, must be deleted.

CLASS III. One parent frustule and a single sporangium.

This is the most common mode of conjugating with the diatoms, as well as the lowest in the scale; and, necessary with all true filamentous species. I select, however, for illustration here, a free form, *Cymbella cuspidata*, and the various phases are represented in Plate V. Fig. 1, shows the frustule imbedded in its gelatinous sac, just before and ready for, the act of reproduction, and Fig. 2, the same diatom, the endochrome differentiated, showing two well-defined globular masses. Fig. 3 shows the same somewhat more advanced, and as seen in side view, and fusion already commenced at the distinct clear space between them. A little later, by increase in the breadth of the internal mass, the frustule opens along the line of suture as shown in Fig. 4, in front, and Fig. 5, in side view, and now, the sporangium begins to increase in length, Figs. 6, 7, 8, and until the final result is the production of a completed frustule, and about double the size of the parent frustules, and shown in side view in Figs. 10, and 11. Here we have the internal contents differentiated, reunited, and the conjugative act all performed, with one individual. I consider it the lowest type. In the next higher, (Class 2) the differentiated substances of two individuals, become, not by reuniting in the same frustule, but by union

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of the reciprocal portions of each, as they lie side by side, the origin of two sporangial frustules; and in the highest type (Class 1), the endochrome is not differentiated in either of the parent frustules, but they act simply as individuals, with a real, although otherwise not as yet discovered, sexual difference.

With regard to the office of the sporangial frustule, and the final development of parent frustules from these, it will be better understood after a consideration of the act of self division, which comes next in importance to conjugation, and I have been very fortunate in observing this. The time required for fissiparous division is much shorter than for conjugating, e. g. at 9^h 30^m P. M. I observed a small *Nav. viridis* with a strong division line, so that two individuals were clearly to be recognized; at 8^h 45^m A. M. next day a division line had commenced in one of them, and at 12 M. next day there was a well marked division line in the other, and at 8^h P. M., same day, not only were both divisions strongly made out, but the frustules themselves were now well rounded at their ends, and at 1^h 30^m P. M., on the third day, the main frustules, each with a strong division line in them slipped apart, and, as the division line in these was about the same as that of the original frustule when first observed, we may conclude that the whole time for complete division would be about six days. Another specimen, which for three days had been seen double, showed another division line between 3^h 45^m P. M. and 7^h P. M. in one of the halves. But it was with the large specimens of *Nav. viridis* (*Nav. major*) that I had the most complete study of the phenomena, and the accompanying wood cuts are from off-hand drawings made at the time, as the changes were too rapid to permit use of the camera; the colored endochrome is represented by the shaded portions.

At 10^h 25^m P. M. I noticed at each end of the frustule an infolding of the membrane enclosing the internal substance, and a fine wavy line on one side, Fig. (1). Five minutes

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FIG. 1.

after I focused for the neucleus, which was very indistinctly visible, as it always is with this diatom, but the stellate arrangement around it was very marked. At this time, 10^h 30^m P. M. the division had progressed as shown in Fig. 2; it

FIG. 2.

was deep within the internal substance, and could only be seen by carefully focusing, and the appearance was as though the membrane was infolding, funnel shaped, and I could distinctly see little particles running ahead of the line, and small globules now above, and now below the line. In three minutes more the division line was about three fourths the distance towards the centre, and at 10^h 36^m the line was completed as in Fig. 3.

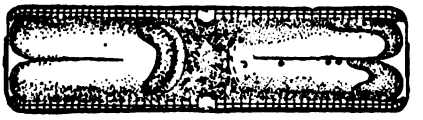

FIG. 3.

During this time slight changes, partly due no doubt to different focusing, appeared in the outline of the colored endochrome, but as the drawings were necessarily made in a hurry, I cannot pretend to any great accuracy in the representation of these.

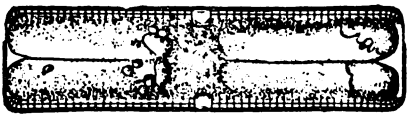

FIG. 4.

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At 10:40 P. M. the appearance was as shown in Fig. 4 and now by carefully focusing on the membrane enclosing the colorless portions of the endochrome, I could see that this was covered with oblong granules, in a tremulous motion, reminding strongly of the so-called "willow leaves" *parva componere magnis*, to be seen on the sun. Small globules were also along the division line which was evidently thicker in some portions than others.

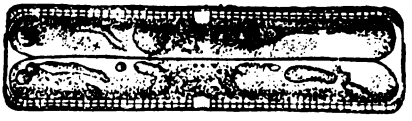

FIG. 5.

At 1^h 30^m P.M. the appearance was as in Fig. 5, a distinct white band crossing the central portion of the endochrome and dividing it, and the division line strongly marked; irregular large oil (?) globules or masses occupied the interior; these, in the course of a few hours, coalesced, and formed four balls in each half of the frustules, two against the central band and two at the ends. I did not observe the final self division completed to the separation of the two individuals of this particular specimen, as it was accidentally crushed, showing to me, however, two things which otherwise might have been unnoticed, viz., the membrane enclosing the endochrome was distinctly wrinkled where it had pressed against the inside of the old valves, but quite smooth at the new division; while the oil (?) globules, floating in the water outside the sac, behaved themselves like ordinary oils, giving a bright image of the light when the objective was drawn back from, and dark when advanced within, the focus. Many observations, made chiefly on the smaller forms, gave for the time for self division of this genus about six days, as already stated.

Much speculation has been indulged in as to the functions of the sporangial frustules. W. Smith and others have imagined that the contents were resolved into a brood of

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young diatoms, and in the "Synopsis of the British Diatomaceæ" two figures are given, representing this supposed resolution; Dr. Pfitzer admits the same in the work already alluded to. I am quite disposed to admit that, as I shall show from my own observations made many years ago, as well as those of my valued friends, Mr. Kitton and the Rev. S. Lockwood, exceedingly minute forms of some diatoms, generally belonging to the group of Nitzschiæ, Naviculæ, or Amphoræ, exist, so minute even that they can, when living, pass through the pores of filter paper; but I have never yet seen any evidence that these can *grow*, i e., increase in size, for the process of self division is one of continued diminution, or, at least, not of increase. Indeed, I am myself quite certain that nothing like W. Smith's "broods of diatoms" has any existence; and, also, that the excessively minute diatoms, which I have seen as well as the skillful observers named above, never develop, by any growth of the frustules themselves, into the larger forms of Naviculæ or Nitzschiæ or other diatoms; but of this presently. So far as the "cysts" of W. Smith, enclosing "these broods of frustules," are concerned, I have found them frequently, and, enclosing in the same cyst, diatoms as widely differing as Gomphonema and Cocconema. It was upon a case of this kind that Dr. Bastian, in his book "Beginnings of Life," founded his supposed resolution of Euglena into diatoms, etc. The Synedræ, figured in the "British Diatomaceæ," Pl. B., Fig. 89, and the Cocconemæ, Pl. C., Figs. iii., iv., v. (221), are not, as he supposes, groups of young frustules resulting from a sporangium or from conjugation; they represent a phenomenon not uncommon, and are the work of a rhizopod (*Biomyxa vagans* (*?*) *Leidy*). I found these cysts of Synedræ in abundance, in October, 1858, often as many as six or eight of them in the field of view at once when using a $\frac{1}{4}$" objective; and the number of diatoms included in the cysts varied from one or two, to fifteen or twenty, and occasionally, mixed with the Synedræ, were frustules of Gom-

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phonemæ. The cysts, when completed and recent, were pale yellow, inclining to red, and full of granules. Plate VI., Fig. 1, shows the appearance of one of them enclosing a number of frustules of *Synedra capitata*, the color and arrangement of the endochrome of these show that they are already dead. The enclosing membrane of the cyst is rigid, and after about forty-eight hours, during which time the Amoeba (or Biomyxa?) has absorbed or assimilated the greater portion of the internal contents of the diatom, the membrane is ruptured, often at several places, and the plasm streams out in a long line through the opening, and then gathers up and begins moving around in search for something more that may serve for its development. It travels along the stems of the confervæ, as shown in Fig. 4, Pl. VI., or glides along the glass slide with a very different movement from the ordinary Amoeba, now sending out long reticulated pseudopodia, now contracting and changing in form; the granules are smaller and the color more pronounced, and the pseudopodia much finer than in the ordinary *Amoeba proteus*. Sometimes as they glide along the stems of the algæ the sarcode is drawn out into long thread-like portions, which are suddenly snapped, then again the mass gathers up; as soon as it reached a number of Synedræ, sessile on the algæ, instantly, and with greatest ease, it spreads itself all over them, as shown in Fig. 3, Pl. VI, and in the space of five minutes the whole assumed the appearance of a perfect cyst. In about two hours, as I repeatedly noticed, the frustules lost the beautiful arrangement of the endochrome of the living forms and assumed the appearance of dead ones; meanwhile the mass of the rhizopod was sensibly enlarged. It is easy enough to see why frustules, mainly of one species, e. g Synedræ or Cocconemæ, should be included in a cyst, as these are sessile, or stipitate forms, densely aggregated, but if a Gomphonema or other diatom should be near by, that would also be drawn in. When, by rupture of the firm wall, the mass has escaped from the cyst, as I have described, the

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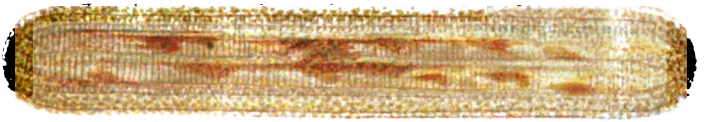
PLATE VI



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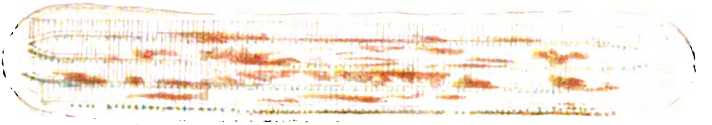
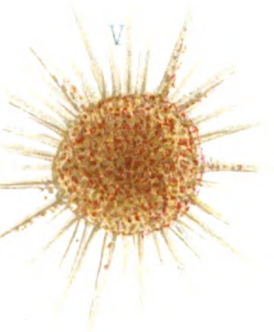


II

III



V

VI

IV

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outline of the cyst is still distinctly visible, as shown in Fig. 2, Pl. VI, and the color and arrangement of the remaining endochrome in the included frustules is very much as shown in the representation in the "British Diatomaceæ," Pl. B, Fig. 89, a mere mass of red patches. The frustules themselves are, however, cemented together as if by partial solution, or deposition of cementing silex, and they cohere in bunches even after acid treatment and burning, but the enclosing cell wall of the cyst disappears. I have many specimens exhibiting this. Sometimes the motion of the rhizopod would continue for five or six hours, after which time, becoming quite globular, it would send out long radial threads, resembling somewhat *Actinophrys sol*, but without the peculiar pulsation vesicles, and frequently, when in this condition, little particles would run up and down the pseudopodia like beads on a string. This phase of growth is shown in Fig. 5, Pl. VI. One of these which I watched for over eight hours, became at the end of this time perfectly quiet, globular and without pseudo-podia, and much swoolen in size as in Fig. 6, Pl. VI; light green particles now appeared in it, somewhat like spores. After remaining in this condition for some time the ball burst suddenly, and the contents were scattered all over the field. I have seen hundreds of these cysts, and sometimes twenty or more in view at one time, enclosing frustules of *Colletonema vulgare*, and I am certain that they have nothing whatever to do with the reproduction of the diatoms, and that the formation of "broods of young frustules" from them is purely imaginary. It has been supposed that the contents of the Sporangium may break up and form masses of micro-gonidia, which in due time may become diatoms, and Rabenhorst has figured something like this; but the immediate and repeated self-division of the Sporangium and the production thereby of normal frustules is not consistent with this view. And moreover, so far as I am aware, it is supported only by the most doubtful observations. I have myself observed the granular

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contents issuing from a broken frustule, somewhat as Rabenhorst indicates for Melosira, but I have never seen anything like the formation of diatom spores. Large zoôspores moving in a dead frustule are not uncommon, and I have numerous drawings representing them; and I have seen them inside frustules that were whole, so far as one could judge from rolling them over and over. These zoôspores are furnished with a long cilium, and their movements inside the frustule are lively. They were conspicuously present in a gathering of *Melosira varians* made in Feb. 1859. The filaments of this diatom showed many enlarged sporangial balls, and in some of the empty cells, frequently in an enlarged or sporangial one, were the zoôspores; and I have even noticed the granules, which for a long time had lain quiet in a dead frustule, to become lively and apparently to form into these spores, and I have also obtained them swimming freely in the water by rupturing the frustule. Whatever these zoôspores may be, I am convinced that they are no phase of diatom growth, they are probably a growth from germs in the water, minute enough to enter at the line of suture or perhaps along the raphè, and which have been developed at the expense of the diatom.

What then is the significance of the Sporangium, and especially an enlarged one? The true answer to this was first given by Braun in 1851, in the following words, "*The strange phenomenon that the primary generation, formed through the conjugation, attains about double the size of the parent cells, is simply explained by a *gradual decrease of size in the series of vegetative generations formed by division.*" The same explanation is given by McDonald, "Ann. and Mag., Nat. Hist." Vol. III, 1869 and by myself in the "Lens" in 1872. I had however, long before this, corresponded with Dr. Greville upon the subject, and found that he held the same views, and his letter bearing date Edinburg, Jan. 5th, 1866, and en-

*"Rejuvenescence in Nature." Henfrey's translation. Ray, Society, 1853; foot note, p. 132.

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closing an illustrative pen and ink sketch, is before me now, and in it he names Prof. Balfour as entertaining the same views.

By the very structure of the diatom frustule, the valves, and the connecting membranes formed inside the old frustule by self-division, *must be smaller* than those of the original frustule, and Pfitzer in his work already alluded to, represents this on a somewhat exaggerated scale; and so it happens that in the course of time, by the continued acts of division of theSporangial frustule smaller and smaller individuals are formed, until at last the size of the original parent frustule, is reached, and at this juncture conjugation again occurs, and a new cycle is recommenced. No doubt myriads of frustules without ever conjugating, go on dividing, and so producing smaller and smaller individuals, theoretically without limit. Whether this be the origin of the minute forms often to be observed in great abundance in the skins coating vessels into which apparently only clean water has been put, is yet to be determined. Some experiments that I have made show that *living forms* which are very minute, may pass through a cloth filter that would arrest dead frustules of the same size, and especially may this happen under the action of light. Whatever may be the origin of minute diatoms appearing under conditions which seem to warrant a development from infinitesimal spores capable of passing even through filter paper, I am strongly persuaded that this explanation of them is a fallacy, and that the sole office of the Sporangium is the one indicated above and not the formation of spores of any kind.

The remarks which I have made as to a return to the normal size after repeated divisions of the Sporangial frustule are inapplicable to forms which do not produce Sporangia longer than the parent frustules, e. g. the Fragilariæ as a general rule, and other genera in which the connecting membrane of the one valve does not overlap that of the other, like the cover of a box, but which simply have the

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edges of this membrane in contact and of the same size on each valve. The greater portion of the diatoms however, do have the *overlapping* cover, or "hoop" as some have called it, and I have repeatedly measured the difference in length of the two valves of the same diatom, e.g. a large *Navicula viridis* (*major*,) caused by the thickness of the overlapping "hoop" or zone. I found in the species just named, that the average length of the Sporangial frustule was .0072″, and of the parent frustule .0038″, and the difference in length between the old and new valves, in the first self-division was, .000125″; and therefore we may infer, that if the division was repeated some thirty times, we would have a return to the size of the parent frustules, if the difference in length continued the same between the old and new valves; probably this difference is less than half in the smallest frustules what it is in the largest, and instead of thirty, more likely sixty or more divisions occur before the frustule is reduced to the normal length, which, on an average of six days, already determined as period for division, would require a year! With our present knowledge however, this estimate can only be considered as a rude approximation, though there is much to warrant a belief, that for the larger diatoms at least, conjugation occurs normally once a year, and approximately, under the same climatic conditions. Among the diatoms the Gomphonemæ make their appearance the earliest. I have often obtained them conjugating from under the ice, and in early spring, every submerged leaf and twig in the running streams is covered with a mucous mass of tangled stipes, full of these diatoms, and forming tufts sometimes an inch in length.

The astonishing increase in numbers by self-division is a matter of easy computation; thus, commencing with one frustule, after twenty-five divisions of the first individual continually repeated, there must be, supposing the new individuals also and their progeny to go on dividing during the interval, 33,554,432 individuals, and after fifty divisions

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of the original frustule, no fewer than 1,125,899,906,842,624 individuals if all should live, but allowing say one-eighth of all the dividing frustules to be destroyed at each successive division, yet even then we would have from twenty-five divisions one million and four hundred thousand frustules nearly, from the original one! It is this rapid increase that makes the diatoms so abundant a few weeks after their first appearance, and in experimenting upon the raising of diatoms in the laboratory, the accidental introduction of one small form may in a few days or weeks give an abundant crop. While therefore I am far from positively asserting that the diatoms do not come from spores, and that they cannot grow i.e. increase in size, I am yet firm in my convictions that this does not occur, nor can I see any necessity for it. The function of the Sporangium appears to be sufficiently performed, in again renewing the cycle, precisely after the manner described, and to have it, instead of doing this, break up into a mass of spores of infinite minuteness, and develop forms by growth of a species which we know from actual observation does in reality become smaller instead of larger by the act of self-division seems to be a departure from the simplicity and harmony we find elsewhere manifested in the growth and development of these minute plants.

When by continued division, long after the normal size has been reached by the diminishing Sporangium, the diatoms become very minute, it is probable that not much, if any appreciable difference can be discerned after the vegetative act, owing to the extreme thinness of the connecting zones, and hence when we find an abundant crop of these minute forms in a "thallus" or layer on the walls of the containing vessel, or late in the summer or fall on the stones in the shallow water, there is not much difference in size, all minute. I have seen some specimens of Amphora, as minute as the smallest germs of the fresh water algæ, and the *Nav. Saugerii* is almost of this character, as also *Nav. atomus*. It is not impossible and indeed in the case of

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Nav. atomus I am pretty certain that I have witnessed it, that these minute forms may conjugate, and that the Sporangium may again do the same. *Cocconema parvum* is a small diatom, and when found is generally aggregated in great quantities in more or less mucous material. I have seen this form conjugating, and the matured Sporangium could not be distinguished from *Cocconema cymbiforme:* the latter I also found conjugating in the same gathering as also often independently and producing a much larger Sporangium very much like *C. lanceolatum.* From some observations I have made upon the filamentous forms, I am induced to believe that as the diatom becomes older, the time required for division becomes longer, and finally ceases, with defective circulation and the frustule dies. In Rhabdonema and Striatella, and to a certain extent Melosira, there are some modifications. I have seen e. g. as many as twelve frustules of the latter in a filament with the halves of the original Sporangial frustules at either end, and yet, the frustules regularly diminishing to the middle of the filament, which could not have happened if they had continued to divide all at equal intervals, giving 2, 4, 8, or 16 as the numbers in the filament at any one time, besides, it is difficult to see how they could in this way decrease regularly to the middle. In Rhabdonema and Striatella, when the interior plates (pseudo-valves) are perforate, there is no difficulty in seeing how the two old valves can be pushed very far apart, with the perforate ones between, but no such structure exists in Melosira. In the ordinary way, supposing we have a frustule to commence with whose valves may be designated a^1, and a^2, the latter smaller and slipping out from a^1, we would have as a result of the first division, two frustules, whose valves would be, a^1 and a^2 slipping out from it for the first frustule, and a^2 with a^3 slipping out of it, for the second frustule; and the four valves of these two frustules in order would be a^1 a^3, a^3 a^2, *i. e.* the old valves a^1 a^2 would be the end valves of this filament of two frustules. Suppose now each of these two, to di-

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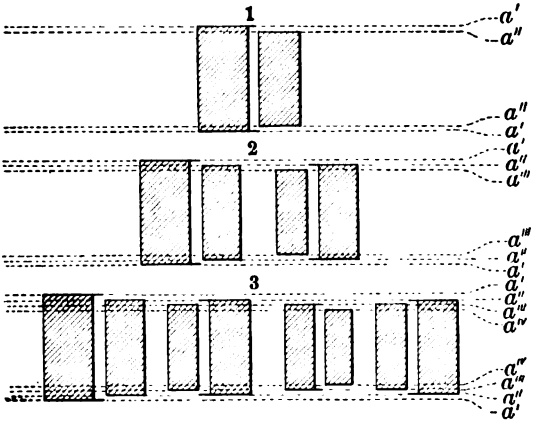
vide simultaneously, we would have four frustules, the valves of the first frustule would be a^1 and a third a^2, slipping out from a^1; for the second a new a^2, slipping out from the second a^2 for one of its valves, and the second a^2 for the other; for the third frustule, the valves would be the first a^3, and a^4 slipping out of it; and for the fourth would be a third a^3 slipping out of the first a^2. Here as before, the old valves a^1 and a^2 would be the end ones of the four frustules in the filament, but two other valves equally as large as the original a^2 would also be in the filament, and the smallest valve of all viz. a^4, *would not be in the middle.* The diagram below will make this plain.



(1) Shows the original frustule with valves a^1 and a^2.

(2) After first division, having a^1 and a^2 for the ends, the new a^2 slipping out of the former, and a^3 slipping out of the original a^2.

(3) Result of second division, a^1 and a^2, original valves of (1) are still the end ones, but in the first frustule, a^1 has a third a^2 slipping out of it, and in the second frustule a^2 has a new a^3 slipping out of it; the third frustule has for its valves the first formed a^3 and the smallest one of all a^4 slipping out of of it; and the last frustule has a third a^3, and the original a^2 as its valves.

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Pfitzer has illustrated this in his plate 6, in the work already referred to. It will appear plain now, that in Melosira, the Sporangial frustule divides, increases in breadth, again divides and so on, the frustules still cohering by the opposed valves, and doubtless some communication between them throughout the chain, though not infrequently this is interrupted; and, sometimes after the division line is formed, the development of one side is arrested, and the new valve, by the increase in breadth of the frustule, is pushed close against the inside of the old one, and this may happen over and over, so that several halves will be formed lying one inside of the other, like a series of empty beaker glasses, and this condition is notably true of *Orthosira Dickeii*, and is not uncommon in some Naviculae e.g. *N. clepsydra* and *N. rhomitica* of Gregory (I am inclined to think that the latter is but *Schiz. Grevillii.*)

The difference in size arising from continued self-division is easily to be noted in a filament of Melosia with a considerable number of frustules. From what has been said it will be understood that in a long chain there will be nodes, so to speak, not necessarily a regular decrease, and when a filament or chain is broken, very likely the smallest frustule may be left at one of the ends. In the chain of twelve frustules of *Melosira varians*, which I have before mentioned as developed from one Sporangial ball, there was a difference of 10 divisions, of the graduated head of a Powell & Leland spider line micrometer, equal to .00033″ between the diameter of the end and of the middle frustules. Although I have never observed any other mode of reproduction, and increase in numbers, than that I have described, it is proper to say, that many persons who have studied these little organisms, and whose opinions are not to be set aside lightly, seem satisfied there *is* some other mode necessary to explain the appearance of diatoms suddenly where none were before to be found, and where it seems they could not have been present except as minute, nearly or quite invisible germs. I have

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already said I am not disposed to admit this, but my testitimony is merely of a negative character, I cannot disprove it, and I shall, myself, add some testimony, which, at first appears to warrant this view: e.g. I found for many successive years the same diatom (a straight form of *Meridion constrictum*) growing in large filaments in rain pools, made at the same place in a meadow in the Spring, but which was dried up through the Summer and Autumn, and the ground baked hard by the heat of the sun. To explain this we may suppose that, as the dry season approached, the diatoms following the moisture retreated farther and farther into the the ground, and possibly remained there dormant in the absence of light, until the succeeding Spring; coming again to the pools formed at this time on the surface, and commencing again a new cycle by conjugation, a cycle for which their long retreat had prepared them. It has been too generally supposed that the diatoms are mere surface forms, and do not live at any considerable depth. I have taken them however in pretty deep water in a sounding cup, and by the dredge in Seneca lake and in the most flourishing condition, and from an examination of the soundings of the U. S. Steam Ship Tuscarora, I am pretty certain that great belts of "diatom ooze" or *living diatoms* exist at depths of from three to four miles, and in fact that at this depth, they are performing the same office in appropriating carbon and eliminating oxygen, as the land plants, and that, without their presence, many forms of marine life now known to flourish in deep waters, could not exist. The constant occurrence of *Gomphonema olivaceum* all over the northern part of the United States in the early Spring, and even under the ice in places where, later in Summer and Fall, hardly a frustule of it is to be found, seems to indicate something like a dormant condition into which they may pass, and from which they may awake again under the rejuvenating power of Spring, and I am disposed to believe, that, after the rapid Spring or early Summer increase, when conjugation has been affected, and to a certain

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extent the vegetative growth has gone on in the Sporangial frustules, reducing them to their normal size, as the warm weather advances, and the waters no longer fill the brooks, and saturate the soil, they bury themselves, and remain more or less dormant until the time returns for a recommencement of the cycle; and as for the marine forms, they may practically, in varying zones of depth, do the same thing, but modified to suit the almost entirely different conditions as to heat and moisture from those experienced by fresh water forms. Anyway, it is an undoubted fact that marine diatoms are more abundant at certain seasons of the year, and that as far as littoral forms are concerned, and these embrace nearly all, they appear to be affected very much as are the fresh water ones. While then, as a general truth, we may accept it that the diatoms disappear largely at certain seasons, whether burying themselves in the moist earth or the soft mud or not, we still find that a great many do remain in the water at all seasons, or even exist on the surface of moist places, and these probably continue the vegetative growth indefinitely or until very minute frustules are produced. It has been stated, on what appears to be good authority, that diatoms which have been dried for a long time will again resume activity upon being moistened. My own experience does not confirm this, I have tried the experiment, but always with a negative result, and so, though I cannot assert the impossibility of doing this, I am firm in my own convictions, that a diatom cannot be dried and again revived. The living forms which have been observed when dried mosses have been subsequently wetted, were undoubtedly conveyed there in the water, and would not have appeared if distilled water had been used though they may with filtered water, inasmuch as minute living diatoms can pass when the dead frustule would be arrested, certainly they may pass quite as easily as spores. Their power of movement under stimulus of light may enable them to get through any pores wherever water can pass, and no doubt they do

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this, and so it is not difficult to get a crop of diatoms from what appears to be pure spring water. That they are in abundance in the ordinary drinking water is sufficiently proven by tying a little bag of Canton flannel over the faucet of delivery, and allowing the water to run through for a while. In this way an abundant crop of diatoms and desmids may be obtained, and frequently other larger fresh water algæ; and a second bag, outside the first, will still show numerous, but minute forms passing through with the first filtered water.

As regards the vitality of the diatoms, I have had some curious experience. In August, 1862, I made a gathering of marine diatoms at New London, Conn.; they remained in the bottle and passed through the putrefactive fermentation, after which the water became perfectly clear and without the least odor; the gathering was mainly *Melosira Borrerii*. The latter part of October, myriads of minute Amphoræ were living, and hanging in festoons from the dead filaments of the Melosira; and in another gathering, made at the same time and place, I found, in July, 1864, two years after the gathering was made, multitudes of minute living Amphiproræ (*A. paludosa?*), and long filaments of Fragilariæ, the latter conjugating; and in May, 1865, in the same bottle, the Fragilariæ were quite vigorous and with some large frustules, and there were many lively Amphoræ of nearly the normal size. I examined it again in January, 1886, and found the same diatoms still living and healthy; but the June following they were evidently on the wane. A part of this time the bottle had been kept in the dark, shut up in a drawer, and once it had been frozen solid. Further observations on this identical gathering were prevented by my removal to Geneva, N. Y., and the bottle was either lost or destroyed. The experiment showed, however, that the diatoms can, some of them, withstand the putrefactive fermentation, which effectually destroys the larger algæ and animal life; and also that these minute forms produce larger ones, not by growth or increase in size, but by repeated acts of con-

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jugation, as witnessed in the conjugation of *Nav. Amphirhynchus*. Among the bottles of the Tuscarora soundings there was one which had, apparently, only sea water in it; it was labeled "Specimen of sea water from a depth of 1,828 fathoms," or over two miles; the cast was made September 22, 1873. I exposed this to the light and heat of the sun for about a month, and I found in November, 1874, a thick yellowish olive deposit, from which minute bubbles of gas were given off, and examination under the microscope showed that it consisted of millions of minute Amphoræ. (*A. veneta?*). The bottle was now put aside and shut off from light until June, 1876. When taken out it showed only the colorless remains of the Amphoræ in the casual dips which I examined, but after exposure to the sun for a week there was a perceptible yellowish tint, and microscopal examination showed myriads of minute green spores, some of a decidedly yellowish tint and conspicuously like a double Amphora, of which there were now a considerable number living. April 5th, 1879, the round spores were still abundant, but not a single specimen could I find of a living Amphora, nor again in June, 1879. Another sounding, labelled "Sea water, 1,015 fathoms," in which I had a long time before found no living specimens, but to which subsequently I had transferred a small amount of the film from the other bottle, has shown, up to the present writing (1880), fine clusters of living Amphoræ and also some moving freely. In March, 1860, I made a gathering of *Gomphonema olivaceum*, and after two weeks' exposure in a saucer, covered with glass, found them conjugating. May 20th the Gomphonemæ were apparently all dead, but there were plenty of living *Amphipleura pellucida* that I had not previously noticed in the gathering. September 15th, the water unchanged, there were multitudes of Amphipleuræ, and also *Navicula acuta*, present and living. In October, and finally in January, 1861, there was a considerable growth of confervæ and a few living *Gomphonema tenellum*. These are a few of many examples

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of which I have notes, but sufficient to show clearly that the diatoms may withstand the putrefactive decomposition, at least the smaller forms, and also freezing, and show themselves living after years. Only in the rarest cases have I found conjugation among these minute forms, and by this act an increase in size; generally they have remained very minute, and the growth was evidently a vegetative one. How long this can go on under proper circumstances it is impossible to answer now. Under the action of light it may be very rapid, but in darkness it is probably arrested, and precisely as I have arrested the development of a tadpole by keeping it almost excluded from light in a small aquarium for over six months, (how much longer, if it had not been accidentally destroyed, it would have remained in this condition I cannot say), so these minute forms may in the dark be dormant, until called into active life by proper conditions of light and temperature, when by rapid division they may become so numerous as to show their presence even to the unaided eye. So far as my own observations go, the Amphoræ, and the very minute Nitzschiæ and Naviculæ are the most tenacious of life. I will close this already prolix article with a description of one other experiment — a sort of vegetable photography. Crossing a shallow ford I was tempted to raise a leaf which, as well as the surrounding hard clayey bottom, was colored that peculiar olivaceous tint denoting a layer of diatoms. I raised the leaf, and a very pretty picture of it remained on the hard bottom; in other words, there appeared no diatoms under it. I scraped the surface of the mud, and filled a small bottle with the diatoms as pure as I could get them. Of course, in spite of all the care, there was enough of the clay-mud mixed with them to make the whole mass appear slate-colored when the bottle was shaken. On arrival home I spread some of this mixed mud and diatoms on a glass plate, as evenly as I could and about the thickness of a penny, and then laid upon it several thicknesses of moistened blotting paper; inverting the whole

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and arranging so as to keep the paper moist, I exposed the glass side to the sun, having previously covered this with a bit of lace. In about an hour I obtained a tolerably distinct picture of the lace, the diatoms coming up to the surface wherever the glass was uncovered.

Desultory and incomplete as these observations are, and giving largely but negative results, yet the bare recital of them has called up most delightful memories. Once more I have been rambling along the Ohio streams and wild ravines, and once more I have seemed to inhale the brisk sea breezes and hear the cry of the startled sea birds. I have trodden again the solitudes of the great western lakes along an Indian trail, and then, so busy memory wills it, have been transferred to the charming water lanes of Guernsey. Once more I have clambered over the rocks at low tide and watched the sun go down behind theg reat Atlantic, at Colo Bay; once more collected diatoms after the deep descent to Moulin Huett, and here before me are the very gatherings made so many years ago. These came from Alderney where, cut off by the rapidly incoming tide, I climbed the nearly perpendicular cliffs; and these from Cherbourg, — classic ground for the diatom lover; and last, not least, these tell me of many long delicious summer days among the lakes and cold streams of Switzerland. But with all these are sadder, tenderer thoughts. And so if there be anything of value in what I have said in these papers about the diatoms, I offer it as a tribute to the memories of those now dead, who kindly aided and encouraged me,—Bailey, Gregory, Arnott, Greville, Eulenstein, De Brebisson, and others less known to fame, but not less dear.

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EXPLANATION OF PLATE I.

Navicula major, in the indigo field; showing the currents produced along the valves; the formation of the little balls of indigo; and the investing mucous material.

EXPLANATION OF PLATE II.

Conjugation of *Surirella splendida;* the fusion is at the smaller end of the frustules, and one frustule has already opened by the swelling of the internal contents.

EXPLANATION OF PLATE III.

The sporangium of *Surirella splendida* after the complete fusion of endochrome; the valves of the parent frustules adhering to the mucous mass.

EXPLANATION OF PLATE IV.

Conjugation of *Navicula firma.*

FIG. 1.—Differentiation of the endochrome in the parent frustules.
FIG. 2.—Formation of the sporangia, with rugose sheath.
FIG. 3.—View of same endwise.
FIG. 4.—The sporangia fully developed in their rugose sheaths; the valves of the parent frustules attached.
FIG. 5.—A single completed sporangium.
FIGS. 6-7.—The completed frustule (sporangial) in side and front view, and after the rugose sheaths have disappeared.

EXPLANATION OF PLATE V.

Conjugation of *Cymbella cuspidata.*

FIG. 1.—Parent frustule.
FIG. 2.—Differentiation of the endochrome in same.
FIG. 3.—Refusion of the differentiated portions.
FIGS. 4-8.—Phases of growth of the sporangium in its mucous envelope.
FIG. 9.—A completed sporangium with the valves of the parent frustules adherent.
FIGS. 10-11.—The completed (sporangial) frustule.

EXPLANATION OF PLATE VI.

FIG. 1.—Group of *Synedræ*, encysted by an amoeba.
FIG. 2.—Same after the escape of the sarcode of the amoeba, through the fissue in the wall of the cyst.
FIG. 3.—The encysting, before the formation of the firm boundry wall.
FIG. 4.—The amœa (Biomyxa,) travelling along a stem of conferva.
FIG. 5.—Same after assuming the ball form and showing the radiating pseudo-podia.
FIG. 6.—Same after several hours quiet, and showing formation of small green spores.

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I. MICROSCOPICAL TUBE-LENGTH,

And the Parts Included in it by the Various Opticians of the World.

II. THE THICKNESS OF COVER-GLASS

For which Unadjustable Objectives are Corrected.

SIMON H. GAGE, Ithaca, N. Y.

I. In the construction of microscopic objectives, the corrections must be made for the formation of the image at a definite distance, or, in other words, the tube of the stand of the microscope on which the objective is to be used, must have a definite length. Consequently, the microscopist must know and use this distance or "microscopical tube-length" to obtain the best results in using the objective in practical work.

In order to ascertain the exact distance in millimeters for which objectives are corrected, and the parts of the microscope included in this distance or "tube-length" the following questions were submitted to all the opticians of the world whose address could be obtained: 1. For what "tube-length" do you correct your microscopic objectives? Please give the length in millimeters or inches. 2. Please indicate on the diagram on the opposite page (Fig. 1. of this paper) exactly what parts of the microscope you include in "tube-length." From nearly all, precise and satisfactory answers were received, and I wish to express here my appreciation of their courtesy. The answers received are given below, and indicated on the accompanying diagram:

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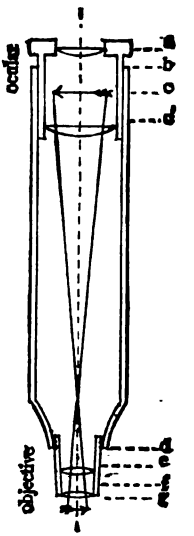

Table giving length in millimeters and figure showing parts included in "tube-length" by various opticians.

Pts. included in "Tube-lengths." See Diagram.		"Tube-length" in Millimeters.
a–d	Grunow, New York	203 mm.
	Nachet et Fils, Paris	146 or 200 mm.
	Powell and Lealand, London	254 mm.
	C. Reichert, Vienna	160 to 180 mm.
	W. Wales, New York	254 mm.
b–d.....	Bausch & Lomb Opt. Co., Rochester	216 mm.
	Bézu, Hausser et Cie., Paris*	220 mm.
	Klönne und Müller, Berlin	160–180 or 254 mm.
	W. & H. Siebert, Wetzlar	190 mm.
	Swift & Son, London	165 to 228½ mm.
	C. Zeiss, Jena	150 or 250 mm.
a–g.....	Gundlach Optical Co., Rochester	254 mm.
c–d.....	Ross & Co., London	254 mm.
c–e.....	R. & J. Beck, London	254 mm.
c–g.....	H. R. Spencer & Co., Geneva, N. Y.,	254 mm.
c–f.....	J. Green, Brooklyn †	254 mm.
c′–e....	E. Leitz, Wetzlar	125–180 mm.
	For oil immersions	160 mm.

*Successors to Hartnack.
†Successor to Tolles.

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A glance at the table and diagram is sufficient to show that there is about as great diversity as possible in the parts included in "tube-length," and that the length in millimeters including these parts, is likewise very diverse. This has, doubtless, come about simply because there was no general standard, and each optician selected for himself a standard. For the sake of those who *use* the microscope, it is hoped that a uniform standard may be chosen, or that, at most, but two standards should be decided on by all opticians. These two lengths in millimeters would probably best be 254 mm. for the long or English "tube-length," and 160 mm. for the short or Continental "tube-length." Furthermore, the same parts of the microscope should be included in the "tube-length," and the parts included should be readily determinable by the youngest student. The parts included by six of the opticians named above, viz: from the top of the tube (*b*) where the ocular is inserted, to the lower end (*d*) where the objective is screwed in, answer this requirement of simplicity. Without urging this as the best possible selection, it will readily be seen that this "tube-length" may be easily measured when the ocular and objective are not in position and that makers of stands who do not also make objectives could easily make the tubes of their microscopes of exactly the right length for the objectives of all objective makers. While it is true that the objectives of various makers are in mountings of different lengths, and, therefore, other things being equal, tend to increase or diminish the actual or "optical tube-length," and thus to vary the magnification of the Microscope, if each maker would choose the length designated above (*b-d*) for which to correct his objectives in their mountings, then no matter how long or short that mounting might be, the microscopist would be able to measure off the right length on the tube of his microsope for which the objective was corrected, and having this length once determined, it would not need to be changed when an objective of different length of setting was used.

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Furthermore, the convenience of the microscopist and uniformity in "tube length" would be both subserved if the eye-pieces or oculars were made "*parfocal*"* "that is the settings be so adjusted that the lower focal points of all the eye-pieces shall be at the same level when in position in the tube of the microscope,"† then no refocusing of the microscope would be necessary upon changing oculars. If also the level of the "lower focal points" of the different oculars were made to fall at the level of the top of the body tube of the microscope, one end of the so-called "optical tube-length" would be always determinable, and correspond with one end, that is the upper end, of the tube of the microscope.

So long as no common standard is employed, it seems to the writer that every objective should be accompanied by a statement and a diagram indicating the tube-length in millimeters for which it was corrected, and showing also the parts of the microscope included in this measurement. If the objective is unadjustable, a statement should also accompany it, giving the thickness of cover-glass for which it was adjusted. (See below under II).

II. *The thickness of cover-glass for which unadjustable objectives are corrected.*

As the thickness of the cover-glass as well as the "tube-length" has an important influence on the perfection of the microscopic image, and as almost all objects for microscopic examination are covered, the objective must be adjustable to compensate for the various thicknesses of cover-glasses used, or some uniform thickness of cover-glass must be selected, for which the optician corrects or adjusts the objective once for all. The thickness for which such unadjustable objectives are adjusted varies with the different

* This convenient name was proposed by the editor of the *Microscopical Bulletin* in vol. iii. (1886) p. 31. See also p. 9, same vol.

† See page 8 of the catalogue sent out by Zeiss, with his apochromatic objectives and compensation oculars. Also *Jour. Roy. Micr. Soc.*, 1886, p. 853.

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opticians, as shown in the table below. The information in the table was obtained by direct inquiry as for the information concerning "tube-length."

Table showing the thickness of cover-glass for which unadjustable objectives are corrected by various opticians:

$\frac{25}{100}$ mm.	J. Green, Brooklyn. J. Grunow, New York. Powell and Lealand, London. H. R. Spencer & Co., Geneva, N. Y. W. Wales, New York.
$\frac{18}{100}$ mm.	Klönne und Müller, Berlin.
$\frac{17}{100}$ mm.	E. Leitz, Wetzlar (when tube 160–170 mm.).
$\frac{16-25}{100}$ mm.	Ross & Co., London.
$\frac{16}{100}$ mm.	Bausch & Lomb Optical Co., Rochester.
$\frac{15-20}{100}$ mm.	C. Zeiss, Jena, $\frac{16}{100}$ mm. (apochromatic oil immersions).
$\frac{15-18}{100}$ mm.	C. Reichert, Vienna.
$\frac{15}{100}$ mm.	Gundlach Optical Co., Rochester. W. & H. Siebert, Wetzlar. R. & J. Beck, London.
$\frac{13-17}{100}$ mm.	J. Zentmayer, Philadelphia.
$\frac{10-12\frac{1}{2}}{100}$ mm.	Nachet et Fils, Paris. Bézu, Hausser et Cie, Paris.
$\frac{10}{100}$ mm.	Swift and Son, London.

A uniform thickness of cover-glass for unadjustable objectives seems also desirable; then by the use of some cover-glass measure, like the one made by Zeiss, the microscopist could easily select covers of the proper thickness to be used for the specimens to be studied with the unadjustable objectives of all opticians.

CORNELL UNIVERSITY, September 1887.

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SHRINKAGE OF CEMENT-CELLS THE CAUSE OF LEAKAGE AND CREEPING IN GLYCERIN MOUNTS.

By Frank L. James, Ph. D., M. D., St. Louis, Mo.

A chemist, thorougly acquainted with the physical and chemical properties of all the known substances in nature, is approached by a microscopist who demands of him a perfect mounting medium for delicate organic tissues. This medium, says the microscopist, must have the following characteristics:

It must be transparent and colorless;

It must possess a high refractive index;

It must mingle readily or dissolve in water or alcohol, or both, and the mixture or solution must be limpid and colorless;

It must be neutral in reaction, and must not affect vegetable colors;

It must not dissolve the resins;

It must be antiseptic;

It must be stable under all ordinary circumstances, not decomposing by exposure to moderate heat or light;

It must be non-volatile in summer's heat, and must not congeal with winter's cold.

In addition to these qualities it should be cheap and easily procurable.

The chemist immediately replies, "I know of one substance, and but one in nature that fulfills all these requirements. Here, my friend, you have it. This substance is propenyl alcohol. It is, as you see, limpid and in thin lay-

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ers without a trace of color. Even in this bottle it has but a faint tinge of yellow or green, part of which is due to the glass. Its refractive index is 1.475. It mingles in all proportions with water and with alcohol, absolute or dilute, and makes a colorless mixture or solution. It is without effect on litmus or turmeric paper, and does not dissolve the resins (such as damar, balsam, etc.). It is antiseptic in a high degree. It is practically non-volatile and unchangeable under ordinary circumstances, boiling only at 554° F. and crystalizing only when agitated and at very low temperatures. It is cheap and obtainable everywhere."

"Give me," exclaims the microscopist, "give me this inestimable treasure! Propenyl alcohol—the very thing I want!"

Whereupon the chemist hands him a bottle labelled "*Glycerin, C. P.*"

The microscopist's jaw drops. "That stuff! not if I know it. *I*'m not going to have a "certain proportion" of my slides ruined. Don't you know that it is an invention and device of Satan himself, contrived especially for the vexation and ruin of the truly scientific microscopist? Don't you know that it has been tried time and again, by our most eminent microscopical editors and professors, and that they have pronounced it the most perverse and willfully deceiving substance in nature? Don't you know that only about one in a dozen slides made with it is permanent? No sir! If you have nothing better than *that* to offer I'll stick to Canada balsam. Propenyl alcohol indeed!"

This imaginary conversation represents the exact status of glycerine in the microscopical technique of to-day. Here we have a mounting medium almost ideal in its combination of those virtues which should recommend it to the microscopist, refused, condemned, almost banished from the work table, on account of its possession, or rather its alleged possession of one grievous fault, viz:

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A tendency to leak or creep out of the cells in which it has been cemented.

That this tendency *does* exist, and that it has been the cause of the destruction of innumerable valuable preparations, and consequently of a vast number of disappointments, I am not prepared to deny. In the earlier days of my microscopical work I too was a sufferer from this "tendency"; but I *do* deny most seriously and emphatically that it is due to any inherent vice or quality in the medium itself, which unfits it for work which is intended to be lasting and stable.

Having thus suffered from the peculiar behavior of glycerine above alluded to, and having at the same time in my cabinet many slides which had remained perfect for so many years that I was justified in calling them permanent, I became convinced that the failures were not due to the use of glycerine. I reasoned that if one single preparation out of a thousand remained permanently good, it was proof positive that with the same materials, manipulated in the same manner, and with the same precautions, all would have been equally so. The laws of Nature are invariable and immutable. Given a certain cause and under the same circumstances, an identical result will always be obtained. With this law in my mind I determined to investigate the causes which led to the varying results which were obtained when identically the same materials were used under apparently the same circumstances.

It is unnecessary to enter upon the details of this investigation, or to report the experiments which finally led to the solution of the problem. It suffices to say that I proved to my satisfaction, as I hope soon to show to *yours*, that the secret lay entirely in *the length of time which was allowed to elapse between the ringing of a slip and the filling and closing the same.* In other words success in making a permanent mount depends entirely (supposing your materials to be what they should) upon the thorough seasoning of the cell-wall.

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The reasons for this are so obvious now that the marvel is that I, or some other worker, had not sooner discovered, or rather reasoned out, the source of error.

All cements used in microscopical technology, with the exception of the ordinary gold-size, consist of a gum or resin, hard and solid at ordinary temperatures, dissolved in a more or less volatile fluid, upon the evaporation of which they regain their solidity. The exception, gold-size, hardens and becomes solid partly, but to a very small degree, by evaporation, but mainly by a process of oxidation. The amount of volatilizable fluid used in these cements depends upon the nature of the gum. In zinc-oxide or white-zinc cement, which consists of gum damar, oxide of zinc and benzol, the latter, in the best quality of cement, exists in a quantity nearly, if not quite, equal in volume to both the other ingredients put together, and hence in its evaporation the volume of the cement is reduced nearly, if not quite, one half.

In other words the cell-wall built with it, in the process of hardening, shrinks to only one half its original volume. This shrinkage, owing to the tenacity with which the damar clings to the surface of the glass, is mainly in one direction, i.e., in its vertical proportions, and consequently its containing power is reduced fully to that extent.

Glycerine is practicably a non-compressible and non-volatile fluid, and it is self evident that if a cell be filled therewith before the walls, by the evaporation of their volatile constituent, have reached a point where further shrinkage will not occur, something must give way. This break will occur at the weakest point of the structure, usually the cover-glass or the line of juncture between it and the cell-wall.

This is plain to everyone. When an irresistible force meets an unyielding body a crash is inevitable. The catastrophy may follow very quickly after the filling of the cell, or it may be many weeks or months, or possibly years in coming, but come it must. The length of delay is

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dependent almost solely upon the degree of seasoning which the cell-walls had enjoyed before being filled and closed.

HOW LONG SHOULD A CELL-WALL BE SEASONED BEFORE IT IS SAFE TO USE IT?

In view of what has been said this becomes a vital question as regards the slide, and a very important one to those who prefer glycerine as a mounting medium, and in order to settle it, for the benefit of myself and others, I instituted a series of experiments which I will briefly describe. I soon found that the external appearances, the looks of the cement and its resistance to the pressure of the nail, did not constitute a safe criterion, especially in large and deep cells. The surface of these might be of glassy hardness, but when they were cut into the interior would be found of almost putty-like consistency. I therefore devised, from the debris of an old aneroid barometer and some other portions of apparatus at hand, an instrument for measuring the amount of shrinkage which a cell of given dimensions would undergo within given periods, and under known conditions as to temperature, etc. The construction of this instrument is as follows:

The base consists of a T shaped bed-plate of brass, each arm of which is about 4.5″ long, 1.5″ wide, and 0.3″ thick. It stands on three legs. The transverse arm carries the slide, rigidly secured by screws arranged at each end. Two levers are so geared that the vertical movement of a rod is shown, multiplied one hundred times on the scale. These levers are perfectly balanced by wrapping fine wire on the shorter arms. Standards curved so as to economize space, are so arranged that by means of two screws they may be moved up or down within minute limits, thus enabling me to bring the index to the zero of the scale without interfering with the gearing, at any stage of a test. The bearings of the fulcral pivots, are knife edged. The scale is of ivory, originally divided into fiftieths of an inch, but subdivided by me, by the eye alone, into one hundred and fifty to the inch.

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Such in outline is the description of the instrument, which, while scarcely adequate for very exact work, answered the purpose for which it was intended. It was constructed out of materials ready to hand or easily converted to serve the desired end. The method of using it is self suggestive. A cell was spun on an ordinary $3'' \times 1''$ slip and allowed to dry so as to become 'tacky' to the touch. A cover-glass, the thickness of which I had previously measured by means of a micrometer calliper, was then taken, and the index rod, removed from its bearings for the purpose, was cemented by marine glue to its exact centre, or as nearly as possible thereto. The cover-glass was then applied to the cell-wall and allowed to attach itself by its own weight or by very slight pressure. The slip had also been accurately gauged as to thickness, and after guaging the thickness of the same with the cell and cover-glass, by subtracting from the result the sum of the thickness of the cover-glass and slip, the remainder gave the initial thickness of the ring of cement whose shrinkage it was desired to measure. The slip was then put in place and rigidly secured by the screw at each end, and the index rod was put on its bearing. After bringing the index to zero the instrument was put under a bell-glass so arranged as to allow a free circulation of the atmosphere the of room and yet to protect it from interference or injury. (Unfortunately for me, however, it did not prevent the fall of a mounted deers-head and antlers, and the smashing into smithereens of the glass and instrument, and the serious injury of my Nachet stand, which occured some months afterwards). The following are the results attained:

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Initial Depth of Cell Wall, =0.020''	Periods of Observation and Record.							Remarks.
	1 day.	2 days.	3 days.	10 days.	20 days.	30 days.	60 days.	90 days.	
Average Temperature, Fahr.....	68°	68°	70°	70°	70°	70°	72°	75°	These are the temperatures between observations, and embrace parts of May and July.
Total Shrinkage up to each observation.....	.001	.002	.0025	.0040	.0052	.0060	.0065	.0070	
Percentage of Shrinkage....	5	10	12½	20	26	30	32½	35	Showing total diminution of 35 per cent. in 90 days.

Owing to the unfortunate accident above referred to, a second series of measurments was interrupted in the sixth week, and I have been so closely occupied since that time that I have had no opportunity to reconstruct the apparatus. So far as it went, the second series absolutely confirmed the first in every particular, except that the weather being hotter and dryer, evaporation progressed much more rapidly.

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The cement used in this series of observations contained originally about 40 per cent. of volatile matter, but had, very propably, lost a small portion of this by evaporation from the container before the series commenced. When the slide was taken off the instrument and the cell cut into, while the cement was not pulverulent when scraped, it was quite hard and dry, and would in all probability have shrunken but a very slight trifle more, if at all, under a prolongation of the tests.

Two things must be remembered here in taking the time and rate of shrinkage into consideration, viz: that had the cell been filled with glycerine the rate of evaporation, and consequently shrinkage, would have been much slower, and, on the contrary, had the cover-glass not been applied and the ring left open to the atmosphere all around it, this rate would have been more rapid.

We may therefore say that a cell made of white zinc cement will season sufficiently for safe use in about 90 days where the temperature remains in the neighborhood of 70° Fahr.

NOTE.—The loss of the original manuscript of this paper—its removal from the Secretary's table at Cleveland by somebody who failed to return it, has forced the author to rewrite it amid other and absorbing duties. The table however is taken from the original note-book and is quite accurate.

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ADDITIONAL NOTES ON CERTAIN SPECIES OF ROTIFERA.

By D. S. Kellicott, Ph. D., Buffalo, N. Y.

"*Floscularia* (*Stephanoceros*) *Millsii*." At the Cleveland meeting of the Society (1885) I read an account of this remarkable floscule; my notes were printed in the Proceedings of that meeting at page 48 and following. Mr. P. H. Gosse, on receiving the reprint of the article, very kindly wrote to me concerning it, and in the course of his note says, "I rather suspect you will find it to be only *Stephanoceros Eichhornii* after all; even if the setæ are not 'arranged in whorls,' yet the whole form is that of *Stephanoceros*, far more than of *Floscularia*." I know it is presumptuous for me to publish an opinion relating to these animals contrary to so distinguished a naturalist, and, if Mr. Gosse had seen the rotiferon in question, rather than my imperfect drawing, I would not do so; but he has not, and it can scarcely be sent for examination alive, whilst mounted specimens are unsatifactory, therefore, I venture to restate the case as I understand it, and this after again examining the animal with the above warning before me. After I have done so I must leave it for others to decide whether or not the floscule is a distinct species, and what are its genetic affinities. Before the preparation of the original article I was aware that its "form was that of *Stephanoceros* far more than that of *Floscularia*." The *long* frontal lobes assimilates it to the former genus, while the diagnosis of the latter, as it now stands, has "frontal lobes *short*"; again its setæ are arranged like those of *Floscularia* and not as stated for *Stephanoceros*, viz.: "set diagonally on

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the lobes in parallel bands." These incongruities I pointed out more or less clearly and at the same time designated enough differences, as I then thought — and I am of the same opinion still — to separate it from *Eichhornii*; this I must insist upon.

These differences I will now restate. I find on comparing the floscule in question with *Eichhornii* — using living specimens of both and the drawings by authors of the latter — that: 1, *Millsii* is not so stout as *Eichhornii* with the frontal lobes slenderer and relatively longer; 2, the lobes are clothed with setæ which are not set in oblique parallel bands as they are conspicuously in *Eichhornii*; 3, the setæ are never seen to vibrate like those of the last mentioned species, i. e., "The instant this happens (captured prey attempting to escape) band after band of the setæ lashes at the runaway, a swift wave of motion runs along each band, and the captive is thrown back into the vortex produced by the wreath at the bottom of the coronal cup;" not so, but the non-vibratile setæ simply reach out from each lobe to interlace with those from the adjacent lobes in a fashion similar to several species of *Floscularia;* 4, the setæ, the reverse of *Eichhornii*, increase toward the extremities of the lobes, each of which is terminated by a brush or tuft of long radiating bristles, equal to half the length or more of the arms when fully extended; 5, the tube is not firm and lobed like that of *Eichhornii*, but is of slight consistency and more irregular in outline, similar to those of representatives of *Floscularia*.

In view of these unquestionable differences I can not conclude otherwise than that the two are distinct and that our rotiferon is a good species, for it certainly cannot be any one of the numerous examples of *Floscularia*.

Is our species then a member of *Floscularia* or of *Stephanoceros?* It stands like this: the setæ and tube are those of the former: the frontal lobes those of the latter. The foot is contracted to an exceedingly short, neck-like pedicel or what I mentioned in the original paper as a very short

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pedicel. After removing several from their cases and examining with higher powers and under more favorable conditions, I cannot regard it otherwise than as a true pedicel. Still, I admit it may be interpreted otherwise; yet it is no more problematical than in certain of the *Floscularia* which are not uncertainly classified. In one young example I could find no trace of a constriction. The very long attenuate frontal lobes then alone remain as a genuine character peculiar to *Stephanoceros*, but this is a strong structural character, while the mere set of the setæ is rather a specific than a generic feature, and the foot termination does not vary more from *Eichhornii* than those of *Floscularia* vary among themselves, hence, perhaps less violence will be done to strict generic arrangement to regard this as *Stephanoceros Millsii* than as *Floscularia Millsii*, as it belongs to neither without modification of their limits, and yet is too closely allied to be separated generically, it seems best to change the diagnosis of one to admit it and I suggest that the clause relating to setæ in the characters of *Stephanoceros* shall be dropped, and instead of "setæ set diagonally on the lobes in parallel bands," it shall read "Setæ throughout the lobes, very long and radiating or set in oblique parallel bands." With this emendation the species should be known as *Stephanoceros Millsii*.

The female eggs are often seen in the tube, several at a time. The dimensions just before hatching are as follows: length $\frac{1}{300}$ of an inch, width $\frac{1}{500}$ of an inch. Just before the young leaves the egg, the two eyes are very conspicuous. The animal is far more abundant than *Eichhornii* in the two streams in which it has been found, namely, Black Creek, Canada, flowing into the Niagara opposite Grand Island, and a small sluggish, stream filled with water lilies, bladderworts and the like, on the eastern side of Grand Island and known locally as "Nigger" Creek. It is, in fact, as numerous in in these places as *Floscularia campanulata*. *F. ornata* and *F. cornuta* are also its companions.

[Examples of *Eichhornii* and *Millsii* were exhibited, side

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by side, to many members of the Society at Pittsburgh, and all conceded the distinction of the latter. D. S. K.]

Brachionus conium, Atwood. This rotiferon was described by Mr. H. F. Atwood in the *American Monthly Microscopical Journal*, Vol. II., 102 (1881). It was discovered in the water-supply of Rochester, N. Y.; the water is derived from Hemlock Lake. I have recently identified it from a sluggish creek on Grand Island, Niagara River, and from a small pond filled with algæ at the "Front," Buffalo City Park, where it occurs in some abundance. This has enabled me to study it and make comparisons with its congeners. This note is offered with the hope that it may help to bring this distinct and notable species to the attention and acquaintance of our observers. I wish also to record one or two points not mentioned by the describer, and to raise a question as to its generic relations.

Of the ten frontal spines the middle two on the upper edge are much longer than the others and bent down over the front with their apices turned outwards. Mr. Atwood found but one of these spines thus. The middle posterior spines are situated on a roof-like piece projecting over the posterior aperture of the lorica, and, since its back edge is twisted or set obliquely, the spines are not in a plane parallel with the ventral plate, one turns upwards and the other downwards. It is not unusual to meet with an example with three eggs occupying each one of the three spaces between the four posterior spines. The species is a powerful swimmer and usually goes boring its way and it appears to prefer rotation in one direction, i. e., right handed spirals. It would seem that the twist of the posterior edge is intended to assist this cork-screw motion forwards.

I do not find the surface "reticulated," but set with minute raised points, giving it a stippled appearance under a moderate power. The long dorsal antenna projects from between the longest frontal spines; the ciliated head-lobes and styles appear to be typical of its family. I can discover but one

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peculiarity: on the lower edge of the lateral lobes there is a sub-lobe which projects downward between the two outer spines of the anterior edge of the ventral plate.

The female eggs are of the usual shape, smooth, $\frac{1}{280}$ of an inch in length, and the young, before hatching, exhibit well formed trophi and well ciliated frontal lobes; the frontal spines of the carapace are well seen, so the young must leave the egg looking very much like the adult except in size.

Is this species one of the genus, *Brachionus?* Its foot is not wrinkled as in that genus but three jointed, the first articulation is distinct, the second obscure. In this regard it agrees very well with the second genus of the family Brachionidæ, viz: *Noteus*; again it has a conspicuous eye spot, the latter genus has none, nor is the lorica faceted as the one species is which gives the characters for the genus *Noteus.*

On the whole it seems to me that this rotiferon would better be considered as the second species of *Noteus*, for the pronounced characters of the foot surely separate it from *Brachionus*, while this character and the roughened, dorsally gibbous lorica ally it to the former. The fact that the shell is not faceted ought not, it seems, to separate genera and possibly *Noteus quadricornis* may yet prove to possess, at some stage, an eye.

We must expect some discrepencies as the knowledge of our Rotifera increases. The present arrangement into genera, etc., is based on the species of a limited area—practically Europe, and is not entirely satisfactory for what are now known. Even the species already referred to above viz: *N. quadricornis* does not quite agree with the family Brachionidæ as described, for example, in *The Rotifera* by Prof. Hudson and Mr. Gosse Vol, II., p. 116, we read in the diagnosis of Family XVIII., "Foot long, excessively flexible, wholly retractile, wrinkled, ending in two toes." Describing the second genus, *Noteus*, of this family it is put thus: "foot obscurely jointed," etc., whilst by turning to Plate XVIII.,

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Fig. 5, of the same work, *N. quadricornis* is represented with a foot having as decided joints as are often found in any species of the Rotifera.

Pterodina patina. Prof. Hudson says of the Pterodinadæ (*Rotifera*, p. 36): "They are almost always creeping about the algæ on the sides of the pond. I never but once have seen any other than solitary specimens. I then found swarms of them in the small space in which the sluice gate of a pond worked." This remark suggested to me that it would not be amiss to state how I have invariably succeeded in getting any number of *P. patina* in the winter. At the head of Squaw Island there is a deep bay of the Niagara which is covered in winter by thick ice; through this fishermen cut holes in order to take minnows for bait; they often bring up with their nets *Anacharis alcinastrum.* This water weed I remove to glass jars in my library, and in a short time have a wonderful crop of Infusoria and Rotifera. The species named usually occurring in swarms.

Asplanchina priodonta. I have recently taken countless numbers of this lovely form by using a skimming net on the lake in Buffalo City Park. As found here it differs markedly in shape from the representations in Hudson's Rotifera inasmuch as it is relatively longer, and has a more decided "neck," i.e. it is more truly flask shaped.

Cephalosiphon limnias. I was somewhat surprised to find, on examining the work on Rotifera, already referred to, that this species is comparatively rare in Europe, and, as said, "very partially distributed." I have often found it at Buffalo and in the Shiawassee River, Michigan, it is positively a common and abundant species. It ordinarily occurs on *Myriophyllum* along with *Melicerta ringens* and *Limnias ceratophylli.*

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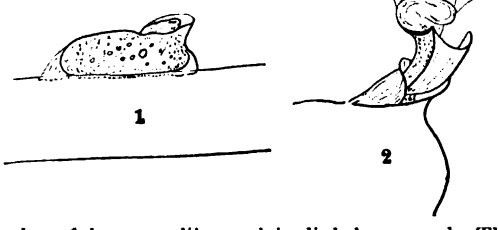
SOME NEW AND RARE INFUSORIA.

By D. S. KELLICOTT PH. D., Buffalo, N. Y.

I propose in the following notes and descriptions to continue the record of my observations on fresh-water Infusoria as published in former volumes of the Proceedings of the Society. The species described by me elsewhere are as follows: *Epistylis ophidioidea*, *The Microscope* IV., 248; *Opercularia constricta*, *Am. Monthly Microscopical Journal* VI., 121; *Podophrya inclinata*, *Podophrya flexilis*, *Carchesium granulatum* and *Opercularia humilis*, *The Microscope* VII., 225. In *The Microscope*, VI., 53, occurs a short paper on certain developmental stages met with from time to time, and, in connection with the papers printed in the Proceedings, I have referred to various rare or interesting species identified at different times.

Lagenophrys singularis, n. s. (Figs. 1, 2). Lorica depressed, convex above, deep brown in old specimens, hyaline in young. The upper anterior portion is prolonged in a gouge-shaped, hyaline semi-tube or neck; above this is a convex, hyaline, membranous lid with rounded outer edge; this lid serves to close the aparture when the disc is withdrawn. The outer edge of the spout-like neck is slightly everted. The animal nearly fills the lorica. The disc is raised considerably above the neck; it is broad, cilia ample, and the hyaline collar or frill, is conspicuous, being as broad as the disc and making a half turn.



Length of shell $\frac{1}{140}$ to $\frac{1}{120}$ of an inch; height or thickness of the same, one third the length; height of anterior prolongation, nearly equal

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to the thickness of the shell. Numerous examples observed on *Myriophyllum*, Scajaquada Creek, Buffalo, N. Y., July, 1887.

The several species of the genus heretofore described are ectoparasites of small crustacea; the occurrence of this one on aquatic plants is, therefore, an interesting exception. As it was found in abundance in all stages there can be no doubt of its preference for plants. With the exception of the recently named *L. labiata*, Stokes, there is no known species with which it could possibly be confused. I am acquainted with the latter and have no difficulty in separating them. *Singularis* is nearly twice the length, its lorica is of a much deeper shade, the neck, while somewhat similar, is quite different in appearance and action. This one is not so shy as those taken on animals, so the disc remains longer and more constant in expansion.

Cyrtostomum (?) *grande*, n. s. Body about twice as long as broad, subcylindrical, exceedingly elastic, extremities rounded, narrowest anteriorly, colorless or slightly brown by transmitted light, white by reflected light. Cilia fine and even. Trichocysts, very conspicuous, in a dense, uniform layer throughout the cortical substance. The mouth is subterminal (armature not fully determined owing to obscuration by the trichocysts). Anal aperture postero-lateral. Nucleus ovoid, granular. (When the endoplasm is separated from the dense cuticular layer the endoplast appears as described, but accompanied by clear globules clinging to its surface.) The contractile vesicles are many, small, pulsating slowly.

Length of body $\frac{1}{50}$ to $\frac{1}{80}$ of an inch.

Pond water among decaying vegetable matter, with *Oscillaria*. Buffalo, N. Y.

I first encountered this remarkable form several years ago taken from the unsavory water of Erie Canal, a mile or more below Black Rock. Repeated visits to the locality at different seasons did not enable me to again find it, until this summer. It has also turned up nearer home in a sluggish stream, or what was once a stream, on Squaw Island, among decaying plants mingled with Oscillaria. It is remarkable for its bulk, exceeding in this respect any of its group met with in this vicinity; indeed its size and force enables it to capture and digest animals belonging to a much

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higher type. I have detected in the same individual the carapaces of two species of Rotifera, viz.: *Anuræa stipitata* and *Noteus conium;* moreover, mingled with these were two shells of a Rhizopod and diverse fragments of Oscillaria, the latter a beautiful deep maroon, due to the acid reaction of the digestive fluids of the endoplasm. If this does not show a voracity which does credit to its kind I am at a loss for a proper example. Here is the equal, in a small way, of the ling, boa or professional gormand. But how does this simple, unicellular creature catch, overcome and engulf *Noteus conium*, rapid and strong swimmer that it is? It would seem that its terrific endowment of trichocysts must have a share in the problem. I shall endeavor to answer this question at some future time.

I have put a query after the genus named for the reason that the characters of the oral aperture and the pharynx remain undetermined. That it belongs to Prorodontidæ there is, I think, no question, so while there may be a doubt as to its genus, its highly elastic body seems to place it with *Cyrtostomum leucus.* It differs from that species in its much larger size and numerous contractile vesicles. Should it prove to be a species of *Prorodon* it must be compared with *P. margaritifer*, but is slightly larger, while that species is without trichocysts.

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Trachelomonas crebea, n. s., (Fig. 3). Lorica spheroidal, varying from decided brown to colorless, elliptical in side view, ratio of longitudinal to transverse diameter as 5 to 4; surface not spined, but with minute, raised points over the entire surface. Anterior tubular projection hyaline, minutely notched and slightly everted at outer extremity, one fifth the length of the shell. The animal is of the usual green, containing many coarse granules; it entirely fills the case; eye spot conspicuous.

Average length $\frac{1}{1000}$ of an inch. Taken in vast numbers from certain ponds in a pasture at Ridgeway, Ontario, August, 1887.

Stein figures the animal of certain species of this genus as crawling through the minute foramen of the case to move

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about freely unprotected. It would seem scarcely possible for so large an animal to squeeze itself through so small a perforation; this suggested to Stein the proverbial camel and the eye of a needle. That under certain conditions they do desert their homes is a fact attested by the following incident: A quantity was left from evening until the following morning in a watch-glass; when on examination I found scores free from their loricæ swimming actively among those still inclosed by their cases. When free they are very changeable in shape like *Euglena*.

The prodigious multiplication of this species defies imagination; the small ponds from which my specimens were obtained exhibited a beautiful green color and had a consistency equal to that of pea-soup. Very few other species of plants or animals were mingled with them.

It is worthy of note that the young cases are not only colorless but smaller than the brown ones, while the neck is sometimes half as long as the shell. Does it grow larger after once formed?

Treatment with hydrochloric acid renders the shell more transparent, causes the animal to greatly contract (the figure is drawn from one thus treated) and renders the flagellum more readily visible. Prolonged masceration in the weak acid removes the extreme brittleness of the lorica so that the flexible walls then readily change their shape by slight pressure from without.

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DETERMINATION OF THE NUMBER OF TRICHINÆ OR OTHER ANIMAL PARASITES IN A GIVEN QUANTITY OF MEAT.

By Simon H. Gage, Ithaca, N. Y.

After meat has been found to be infested with parasites, if it is desired to determine the number in a kilogram, pound or any other weight, a section of the meat is made with some sharp instrument and the thickness of the section is measured by placing it between two cover-glasses whose thickness is known, and then after pressing the cover-glasses quite firmly together, measuring the entire thickness. The thickness of the section of meat is then easily determined by substracting the thickness of the cover-glasses from the number representing the thickness of the cover-glasses and the meat The sections may be from one-tenth to three-tenths of a millimeter thick.*

Put a square diaphragm into the ocular and by means of a stage micrometer determine the value of the side of the square field.† Put the section of meat under the microscope and count the number of parasites in the field, moving the specimen and making twenty or more counts in order to get an average which shall fairly represent the number of parasites in one field. Find the cubic contents of one field by multiplying the thickness of the section by the number

* A very convenient instrument for making all fine measurements like these, is the cover-glass measurer of Zeiss.

† It is of course unnecessary to have a square diaphragm in the ocular, but it is easier for most persons to compute the cubic contents of a solid having square than one having circular ends.

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representing the value of the sides of the square field. From this, compute the number of parasites in a cubic centimeter. Divide this number by the specific gravity of muscle (1.058) and the result will give the number of parasites in one gram of the meat. From this the number in one kilogram may be obtained by multiplying the number in one gram by 1000, or the number in one pound avoidupois by multiplying by 453.593 (the number of grams in a pound). The following is an example of the application of the method:

Number of trichinæ in a pound and kilogram of the muscle of a cat: The thickness of the section was .27 mm. and the value of the side of the square field as seen in the ocular was 1.5 mm. The number of trichinæ in 20 different fields was as follows: 4–3–0–2–2–3–0–1–2–4–8–3–4–5–5–2–4–2–2–4 =60 the average number in each field being therefore 3. The cubic contents of each field was $.27 \times 1.5 \times 1.5 = .6075$ cubic millimeter. Now, if in .6075 c. mm. there are 3 trichinæ in a whole c. mm. there would be $3 \div .6075 = 4.938$ and in a cubic centimeter of meat there would be $4.938 \times 1000 = 4938$. trichinæ. But as the specific gravity of muscle is 1.058, in a gram of meat there would be $4938 \div 1.058 = 4667.3$. In a kilogram there would be 1000 times this number and in a pound avoidupois 453.593 times as many, this would give for a kilogram 4,667,300 and for a pound 2,117,054.

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DISEASE GERMS.

Another Illustration of the Fact that Bacteria Cause Disease.

By T. J. BURRILL, PH. D. Champaign, Ill.

Notwithstanding the numerous demonstrations of the active agency of micro-organisms in the production of disease, especially in animals, doubts are still expressed upon the subject in private interviews and in public prints. If these skeptical or combative utterances did not frequently come from those who are considered, however worthily, to a greater or less extent, authorities upon such matters, further contributions to the exposition of the truthfulness of the so-called germ theory of disease might not be considered necessary or useful. But there are to-day honest skeptics upon the subject who really wish for reliable information. Whether or not they have made reasonable use of the published account of experiments, whether or not they have embraced possible opportunities to experiment and investigate for themselves, need not now concern us. One who is convinced of a truth ought not to stand upon his dignity about publishing and republishing his reasons for the faith that is in him. There are other skeptics in the matter who apparently will not believe evidence on account of some previous occupation of the mind with other notions, the mental capacity perhaps not being sufficient for two ideas at the same time and the mental inertia (if one may so speak) too great for change when once started. As these latter intellects are not to be looked for within the membership of our organization, nothing further need be said here

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to meet such opposition. To the former, if present, this communication may possess something of interest.

We certainly have some advantages in investigations of this kind in dealing with vegetables instead of animal bodies, on account of the comparative simplicity of structure and physiological function in the former. In plants each cell is, in an important sense, an independent physiological unit. One can cut a section from a living plant for microscopical examination and keep it under his eye for hours, while it retains its normal, vital condition and activities. There is no nervous system to complicate the problem and perhaps lead one to false conclusions as to the actual cause of the phenomena presented. In plants there are no so-called constitutional maladies, like ague and fevers. Disease is always local, at least in origin. If other parts are ever involved in consequence, it is only in a mechanical or physical way, as when by the want of proper root action the stem and leaves may suffer from the want of water, and not at all through sympathetic communication. Then again the structure of vegetable tissues is such that the elements can be much more readily examined by themselves. The cells are larger and more distinct as well as more permanent; the difference between wall and contents is greater, and the entire cell-structure is easier identified and examined.

To be sure, comparatively little has been done in the investigation of plant diseases due to bacteria; but this is sufficiently explained by the relatively few workers upon this department of plant pathology. While the earliest known disease of this kind upon plants was first announced in our country and to this society, there have been recorded in America, aside from the writings of the author, the results of but one series of careful investigations upon the relations of bacteria to vegetable disease, and this upon the same subject as that first presented, viz: the so-called fire blight of pear and other pomaceous trees.

This is by no means for want of opportunity upon the

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material side. There certainly are enough plant diseases of the nature in question to furnish abundant chance for investigation. The failure is wholly upon the part of the investigators. Man's body is animal, not vegetable in make up. This in itself is sufficient to give extra stimulus to the studies upon the former kind of structures. There are hundreds and thousands of active workers whose professional business it is to deal with diseases of animals and man, and hundreds of special schools where instruction is directly imparted to students upon their diseases; but the professional vegetable pathologists are yet to be evolved, to say nothing of special schools for their training. Yet the time cannot be far distant when critical investigations will be made upon vegetable structures for the very purpose of helping towards the solution of the problems presented in animal pathology. This has been recognized by Sir James Paget whose paper upon the subject is interesting reading. As a contribution to the general doctrine of "disease germs" as well as for the intrinsic importance of the new results herewith presented, this paper is respectfully submitted.

During several years complaints have been made by those who grow crops of broom-corn and sorghum (*Sorghum vulgare*) of an injury especially evident upon young plants, but also upon those of any age, whereby great losses have occurred. Sometimes the unwelcome appearances of disease are confined to definite but usually irregular areas, and often within these areas the entire crop is destroyed. In other cases the diseased plants, in greater or less numbers and in various conditions of injury, are distributed throughout the field, smaller in size than the healthy ones, if any, and of a general sickly appearance. The lower leaves of the affected plants gradually die, but are first spotted and splashed with crimson-red, in all sizes and shapes. This conspicuous coloring is more particularly observed upon the upper portions of the leaf-sheaths which invest the stem, and, to a less extent, along the mid-veins of the leaves.

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The stems themselves are not commonly damaged locally in a serious manner. If the stalks live to develop the brush or seed panicle, the peduncles or wiry stems of the latter are often badly scarred with irregular reddish patches. These may often be seen in manufactured brooms.

Upon examining the roots many of them are found dead. The affected plants are easily pulled up, often yielding to very slight force, while their healthy neighbors resist a vigorous pull. The oldest roots die first, and, as others are gradually emitted from the lower part of the stem in successive circles in the well-known order with these plants, they become successively diseased and die, so that only the youngest or those emerging highest upon the stem are still alive. There are no abrasions of the surface; as far as can be made out with a hand magnifier the tissues both of the leaves and of the roots remain intact until dead and dry, when the affected parts soon crumble away. Of the roots it seems to be only the exterior layer of tissue or cortex in which the disease is resident.

The injurious effects, now described, have been attributed to insects, to parasitic fungi, to unfavorable conditions of soil and of climate, and to constitutional weakness of the plants themselves. The crops are not comparatively important ones in this country at large and are usually locally cultivated, so that relatively little attention has been given to them by scientific investigators. But an account of careful studies upon the diseased condition of the plants and upon the insects infesting the fields is given in the Thirteenth Report of the State Entomologist of Illinois (1883) by Professor S. A. Forbes. These studies were avowedly unsatisfactory, but finding great numbers of plant lice, of four distinct species, in the fields, it was thought these or some of them probably caused the mischief. If however the injury could be assigned to the lice at all, Professor Forbes concluded that the main damage must have been done before the time of his examination and that the depredating

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insects had largely disappeared, for their distribution at the time did not correspond with the evidence of damage done. He thought the trouble might be due to fungi, and specimens were sent me for examination, upon which a negative report was made.

I am not aware that the injury in question has elsewhere been attributed to specified insects.

In the *Prairie Farmer* for August, 1884 (Vol. LVI., p. 532), I gave a description of a fungus, supposed to be an unnamed species of Chætostroma, found in abundance upon affected leaves of broom-corn. This seemed at the time to cause at least some of the damage noted. It has been found several times since and probably does injure the crop, but cannot be connected in any way with the main disease with which we are now concerned. This completes, so far as I am informed, the accounts that have been published having reference to the particular and conspicous injury at present under discussion. If the references really are complete, we can understand that very little has been known upon a disease which has certainly existed at least six years in our country, and which was probably imported from abroad with the affected plants many years ago.

In July, 1886, I collected for microscopical examination some of the diseased plants and, upon using higher magnification than formerly, found numerous bacteria within the cells of the affected parts. An assistant in the Illinois State Laboratory of Natural History, Mr. Charles Woodworth, was then asked to make special experiments and observations. The results were speedily convincing that a specific microorganism was in some way connected with the disease, and apparently as an active agent in its production. But for some reason, still unknown, after August 1st, 1886, the culture experiments undertaken were not successful, and the matter was dropped for the time, on account of other work. A pure culture of a Bacillus had, however, been made from

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the affected plants, and the disease was experimentally produced by the use of this artificial culture. Sterilized water was used in exactly the same manner as a check and always without result.

Wishing to ascertain the conditions of things in the winter, some broom-corn seed was planted in February, 1887, in the green-house, and April 6th, 1887, Mr. M. B. Waite, a student under my advice, commenced inoculation experiments upon the young plants with material taken from an old diseased stalk obtained at the time from the field. This old stalk contained, in great numbers, living microbes, though it was frozen hard when collected. There were also many spores recognizable by their peculiar refractive properties and uniform shape. The cultures from this old material were not always pure; but the prevailing organism was a Bacillus of apparently the same microscopical and culture characteristics as that found the year before. This organism was easily obtained in a state of purity by plate cultures and also was found to be pure in several direct transfers from the old stock, care being taken to effect this result.

The inoculation experiments upon the young plants were at once successful, both from macerations of the old diseased stock and from pure cultures of the Bacillus mentioned. Checks were made upon the same or similar plants by the use of sterilized water and of stertilized beef-broth like that in which the cultures were made. Studies were prosecuted until the first of June, when they were again interrupted, to be resumed a month later, by myself, aided by Mr. Waite, who had now become an assistant in the Illinois State Laboratory.

Without pausing now for the records of single experiments, a general summary of results is presented.

In the described disease of broom corn and sorghum a specific Bacillus is constantly found in the affected tissues, both of the roots and of the aerial parts of the plants. Pure cultures of this Bacillus may be made in beef broth, and in

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infusions of potatoes and maize kernels, as well as upon nutrient gelatine and agar agar. The best growth takes place at a temperature of about 36° to 37° C., but development proceeds more slowly as the temperature is reduced below 25° C. Still lower temperatures were not tested. In potato infusion in a test tube, inoculated with a minute amount of a previous culture, or directly from the diseased tissues, and and placed in incubator at 36° C., the limpid fluid becomes sensibly turbid in twelve hours, and in twenty-four conspicuously so. In the latter case spores may be sparingly found. These are produced in a characteristic and uniform manner, one in the middle of each cylindrical segment of a chain, or in the individual microbe if separated. There is some undetermined condition under which the chains of cohering organisms are formed, for in some cases this is the prevailing form, while in others, apparently similar, the individuals are separated. In the most active stage of growth—say at twelve hours under the above conditions, the organisms are almost uniformly in pairs. In the preparation for spore-formation, changes take place in the protoplasmic contents of the cells indicated by the action of staining agents. During active growth methyl violet stains uniformerly and deeply the whole body. When spore formation begins, the central area of each cell is noticeably paler. At first this lighter colored portion looks like a pale, indistinct, equatorial band or zone without distinct limitations. Gradually the differentiation becomes more pronounced, until one sees a cylindrical cell with a dark spot in each end, and a comparatively large, white central area. By very careful examination, however, an exceedingly delicate film, seen in optical section as two fine border lines, can be made out as a lining of the cell wall throughout the white area. Sometimes the dark end spots appear like circular dots, but usually conform to the external shape of the cell

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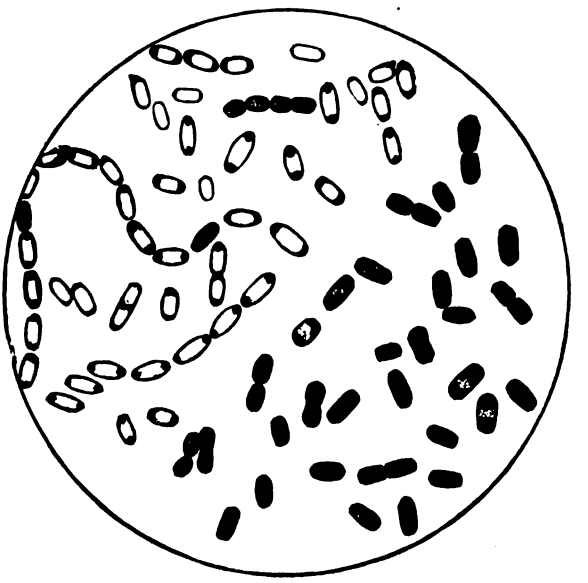

and are concave on their inner sides, or on those looking towards each other. These spots grow gradually smaller but do not wholly disappear, at least in many cases, until the cell-wall dissolves and leaves an oblong free spore entirely colorless except perhaps at the ends, where the violet stain still leaves its mark. When still older this agent does not color the spore. Aniline red with carbolic acid does stain them. The Bacillus averages .7μ in transverse diameter, but varies from about .5μ to 1μ. The joints are short, but run from 1 to 3μ in length, 1.5μ being more common. When as pairs newly divided the segments are oval but usually the shape is short-cylindrical. As the spores form, the sides become convex, so that the outline is elliptical, the ends however, remaining obtuse, or semi-circular. During the period of active growth, the organisms have flagellate motions, but these are never very rapid compared with many others, neither does the power seem to be long retained.

On plate cultures the characteristic growth is white or

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pearl-like with peculiarly lobed and fimbriated margins. Gelatine is not liquified. In liquids, in the incubator as described, a pellicle forms upon the surface within twenty-four hours, but afterward becomes thicker. It is white or nearly so, usually polished or glazed above, with characteristic granules and pits. The growth extends upward on the sides of the test-tube about three millimeters. After a time the pellicle becomes brittle, easily breaks up and gradually settles to the bottom as a floculent precipitate.

When some of the culture fluid filled with the microbes is smeared upon the upper or under surface of healthy leaves of broom-corn or sorghum, after forty-eight hours minute red specks can be seen by the unaided eye. These specks are usually thickly dotted over the entire area to which the application was made; but, sometimes when the fluid settles more upon a given portion of the leaf, the spots are conspicuously more abundant there. By the aid of suitable magnification, it can be readily determined that the minute red specks owe their location to the stomates or breathing pores of the leaf. If a leaf is previously marked off by ink-lines into equal areas, say two inches wide, and, upon alternate blocks is painted in one case a fluid containing the Bacillus described, and upon the other a sterile fluid of the similar kind, the results are very striking and convincing. We find the former set speckled throughout with the red dots. At a later time, say four days, the peculiar checked appearance of the leaf corresponding to the treatment named, is conspicuously manifest, though not all of the tiny red dots subsequently enlarge. The infected surface becomes irregularly blotched. If now a portion of such a diseased leaf is slightly flamed, then cut with a flamed knife so as to divide the epidermis, and start a crack in the tissues when the leaf is bent, a freshly *broken* exposure of the diseased parts can be secured. A glass pipette just from the flame can now be thrust into the newly exposed reddish tissues and a culture started with pretty strong pre-

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sumptions that whatever growth results, comes from the infected leaf. In this manner, time after time, a pure culture of the specific Bacillus has been secured and from these cultures the disease has been again started. Moreover the Bacillus itself has been found numerously enough in the affected tissues both when the disease occured spontaneously, that is naturally, and when artificially started as described.

But I have not yet learned to exhibit satisfactorily bacteria *in situ* with vegetable tissues. No stain or method of discoloring has been found servicable in these cases. Then too, the sections must be so thin that the cells themselves are usually cut and the contents escape. If infiltrated the mass interferes, or if the latter is dissolved the organisms are likely to be washed away.

If sections are made of a newly infected leaf, it is easy again to demonstrate that the disease starts at the stomates. The guard cells themselves may or may not be changed, but the cells immediately beneath the aërial cavity show the initial influence of the disease. From the stomates the injuries spread downward and sidewise. In thus spreading originally distinct spots coalesce and form continuous blotches as described.

The cell-walls are in no wise altered so far as can be made out by microscopical examination, except that they are stained throughout with red instead of their normal clear white. The first change observed in the cell-contents is a shrinking of the protoplasm, as when treated with alcohol. It separates from the wall of the cell and appears rigid instead of the usual plastic consistence in health. The chlorophyll granules, if present, lose their green color and break up into granules of much smaller size. The mass continues to shrink and becomes tinted with red. From this time on, the change does not appear to be always the same. Sometimes the shrunken mass seems tough and remains like a lump in the middle of the cell. In other cases it breaks up into granular débris, immersed in water.

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If starch grains existed, they share the fate of the other material and are decomposed. At length the substance passes into what seems to be an emulsion of oily matter in water. The spherical particles are dark red and usually exhibit Brownian or molecular motion. In certain cells minute starch grains, of uniform size and shape, like little double-convex lenses, occur in great numbers and oscillate rapidly in the watery cell-fluids. These may be easily mistaken for microbes, but iodine stains them blue. They have been observed only near the borders of the diseased areas, within the parts which have turned red. Finally little or nothing remains within the inclosure of the cells; the surfaces of the wall, however, usually show what seems to be a granular or dirty deposit clinging to them. The cellulose of the cell-walls is stained beyond the area penetrated by the the microbes. The liquid itself in the diseased cells is red, colored thus by some soluble substance, perhaps a compound of tannic acid.

No attempts have been made to ascertain the chemical changes that take place either in the cultures or in the plants; but it is evident from what has been said that the injuries are chemical rather than mechanical. As before indicated the effect is, at first at least, purely local. It seems, however, quite probable that cells adjoining the invaded ones sometimes suffer from the absorption of the liquid only of the diseased parts. It indeed may be true that the protoplasm which shrinks into a lump and remains unchanged otherwise in the cell, is killed by the poisoned liquid and not by the direct action of the microbes. The latter have not been observed in such cells. The red coloring matter is not directly elaborated by the organisms, but results from the chemical decompositions of the cell-contents. The bacteria themselves are white and do neither absorb nor excrete the red stain. No culture fluid tried is changed in color by them.

The question is sure to be asked, "What are you going

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to do about it?" The so-called practical man is very apt to say, "I care nothing about the cause. Tell me the cure." It is not always easy to say how an enemy can be safely met when he is seen; but it is true that a known foe is himself more exposed than one who fights under cover. The knowledge of the cause may lead to the cure.

If we concisely review the facts, remedial practices may be suggested. The destructive organisms infest any or all parts of the plants and live over winter, either as spores or otherwise, in the old material. In the case of broom-corn there is a large mass of this old substance left upon the ground, and it has been observed that since the improvement of plows permitting this refuse to be turned under, the disease has been much more destructive than when it was necessary to clear the ground by burning the old stalks. Undoubtedly one thing to do now, is to return to the former practice, and carefully burn the stubble. But this does not dispose of the old roots and underground parts of the stems which are infested with the parasites. Rotation of crops is better than trusting to burning, and field practice has given excellent demonstration of the utility of this system of management. Crops are sometimes injured on land not previously planted with sorghum or broom-corn; but the injury is invariably greater, so far as direct observation has been made, when successive crops of these plants are grown on the same land. The same microbe does not appear to affect maize, wheat or oats, though it is probable that it does live and prey on some other members of the great grass family. The most serious damage is done to the roots and no doubt these are far more liable to be infected from organisms already in the soil, than from such as might be washed down into fresh ground by rains. If the soil, on the other hand, contains great numbers of the living microbes, many of these will get into the air by the evaporation of water from the infested earth. This fact has been disputed, but experiments have repeatedly shown that bacteria may

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be carried over in the practice of distilling water as well as disseminated in natural evaporation. Whether they ride on tiny droplets or are simply moved by the aerial currents produced, we need not stop to inquire. Their own power of movement is of course useless for such dissemination as we now consider.

It is quite possible that special fertilizers may be of service in checking the ravages of this disease, but nothing upon that subject is known. There seems to be no difference in regard to the general fertility of the soil. Indeed, the more luxuriant the growth, the more conspicuous the appearance of the disease upon artificial infection. Moist weather also seems favorable to the spread and abundant development of the malady. Rains appear to be efficient agents in the natural infections. During the unusual dry weather of the present season, the crops suffered little comparatively in the particular manner in question. Nothing has been noticed as to the difference, if any, in the different varieties in regard to the liability of infection Very probably there is some variation in this respect.

According to the tenth census of the United States there were produced in 1879, 12,792 lbs. of sugar and 28,444,207 gallons of molasses from sorghum and 29,480,160 lbs. of broom-corn. Counting the sugar at five cents per pound, the molasses thirty-five cents per gallon and the broom-corn at one hundred dollars per ton, and estimating the loss from this disease as five per cent. of the entire sum, which is certainly far within the actual amount, we have $571,506.00 as an annual tribute laid upon these comparatively unimportant crops in our country by the microscopic invaders, belonging to a single company of the mighty host which we are just beginning to recognize as warriors and enemies. Is it not time that we were opening our eyes and bestirring ourselves for a determined engagement? Victory ought to be, and may be, ours.

Having now presented the chief results of these investi-

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tions, permit me to recall attention to the fact that this plant disease is started by simply placing the living microbes on the healthy leaves. There is no wound of any kind as a prerequisite. There is no preliminary letting down of constitutional vigor. The action of the bacteria is direct and indisputable. They, and they alone, must be considered the agents of the injury. It is by no means asserted that this is the first time such proofs have been offered, or that the work in this case has been exceptionally well done from the standpoint of general bacteriology; but, upon the principle of line upon line, the results must help to strengthen testimony already strong.

It only remains to introduce the new member of the family of known microbes as *Bacillus sorghi*, n. sp.

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ENDING AND RELATION OF THE MUSCULAR FIBRES IN THE MUSCLES OF MINUTE ANIMALS.

(Mouse, Mole, Bat and English Sparrow). -- Abstract.

By Susanna S. Phelps Gage, Ph. B., Ithaca, N. Y.

The objects of this paper are to show the length of the fibers in animals so small that none of the muscles are 40 millimeters in length, and also the form and relation of the fibres.

1. In the shortest muscles, as the intercostals and the penniform muscles of the limbs, the fibres extend from tendon to tendon (demonstrated in mouse and sparrow).

2. In some of the longer muscles (4 to 16 mm.) from both trunk and limbs of the mouse and sparrow, a few fibres were traced from tendon to tendon, as with the shortest muscles.

3. In the longer muscles (4 to 27 mm.) from both trunk and limbs of mouse, mole, bat and sparrow, were also found fibres, which, arising either at the tendon of origin or insertion of the muscle with the familiar blunt endings, do not extend to the other tendon, but end within the muscle with a more or less tapering extremity. No fibres were found without at least one end connected with either the tendon of origin or insertion, that is, there were no spindle shaped fibres with both ends within the muscle.

4. The fibres having endings within the muscle were found to be from one half to two thirds the length of the muscle to which they belonged, that is they vary from 3 to 18 mm.

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5. The endings within the muscle are of four types: (a) long tapering, unbranched endings (or with only fine processes); these occur frequently in mouse, mole, bat and sparrow. (b) Dichotomously divided endings, both divisions being striated. These were found in mouse and sparrow. (c) Endings which have many striated branches, recalling the tree-like branching of the muscles in the rat's lip and frog's tongue. These were found in mouse, mole and sparrow. (d) Long tapering endings with an occasional short, thick, striated branch; found in mouse and mole.

6. Fibers when of full size were found to give off large branches, (a) within the muscle in mouse and mole, (b) near the tendinous end in mouse and bat.

7. The connection of the endings within a muscle with other fibres is frequently very intimate, and true anastomosing, striated branches were found connecting two contiguous fibres (the anastomosis of fibers demonstrated in the mouse).

8. From the above facts it is seen that in these minute animals (a) in the shortest muscles, as in the larger animals, the fibres extend from tendon to tendon; (b) in the longer muscles, some fibers extend from tendon to tendon, in this respect differing from the larger animals in which the fibers are rarely, if ever, more than 40 mm. long; other fibers arise at a tendon and end within the muscle as with the larger animals, but there are no fibres such as are found in the larger animals with both endings within the muscle; (c) such anastomoses of one fibre with another as occur in the mouse, has not, to my knowledge, been described in any other animal.

9. The form of the endings within the muscle recall the cardiac muscular fibers of cold-blooded vertebrates and of mammalian embryos, while the anastomoses of the fibres, in the mouse at least, suggest the cardiac muscular fibers in adult mammals; hence it seems as though the difference between the skeletal and cardiac muscular fibres may not be so radical as has been supposed.

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THE BACILLUS OF FOOT-ROT IN SHEEP.

By Mark Francis, D.V.M. State University, Columbus, Ohio.

The prevalence of contagious foot-rot in sheep in Ohio, the fact that Ohio is a wool-producing state, that the animal loss to the state is to be estimated by thousands of dollars, that the specific nature of foot-rot has never been investigated, that remedies consist in the use of mineral acids and alkalies that are highly caustic in their effects, all these combined, induce me to consider this disease a fit subject for investigation with a view to a more rational system of treatment.

Accordingly, January 10, 1887, the investigation was begun in the following manner: A piece of white silk thread known as surgeon's silk, was infected with the discharge which issued from between the toes of a sheep having foot-rot, in Jefferson county, Ohio. The thread was then carefully dried and sent in a letter to Dr. H. J. Detmers, Columbus, Ohio, thus exposing it to the temperature of January. On February 13, 1887, small pieces of the thread, say one third inch long, were put into sterilized gelatin culture tubes which were then allowed to rest obliquely.

The gelatin culture tubes were prepared in the following manner: Pieces of meat, of young animal preferred, were boiled down to a jelly at a temperature of about 165° F. for six to eight days, then filtered in a water-oven into sterilized test-tubes, say two inches deep, and the tubes closed with a plug of absorbent cotton which had been thoroughly disinfected. The tubes were heated two hours daily for five days

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in the water-oven at a temperature of 165° F. and allowed to cool at an angle of 45°.

One of the tubes seemed to be contaminated with a fungus and was discontinued. The other presented a delicate white growth near the thread and on the surface of the gelatin which showed two things. First, that the bacteria are incidental parasites by living on the thread and, secondly, that they are aërobic, that is, that they need the oxygen of the air for their development; this explains why they remain as a local source of irritation and do not enter the blood stream and produce general disease.

On February 27, 1887, another culture tube was inoculated from tube No. 1, in the following manner: The culture tube was held in the left hand in a horizontal position, the plug of cotton removed with the right hand and held between the fingers of the left hand so that nothing could fall on it. Then a platinum wire, that had been disinfected by heating in the flame of a spirit lamp and allowed to cool, was introduced, and a small quantity of the gelatin adhered to the wire, then the cotton was replaced and the material was introduced into the second culture tube while being held as before. Cover glass preparations were then made in the following manner: A very small drop of distilled water was put on a clean cover-glass and with a platinum wire, having been first heated in a spirit lamp, a minute portion of the gelatin culture No. 1 was taken from the tube (held almost horizontally) and mixed with the drop of water on the cover glass, and so distributed as to make a thin, even spread. Then with a pincette the cover-glasses were placed under a bell glass and allowed to dry. When dry they were taken with the pincette and passed quickly through the flame of a spirit lamp three times, bacteria side up, then dropped into a watch glass containing a solution of Bismark brown or vesuvine that had been previously filtered.

The covers were allowed to float thus in the staining

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fluid from five to seven hours according to the intensity of the stain desired. When sufficiently stained they were taken with a pincette and washed, either passed quickly through a beaker of water by a to-and-fro motion, or washed with a wash bottle until the water came away clear, and again put under a bell glass and allowed to dry. When dry they were mounted in Canada balsam.

Several cultures were made at intervals of a week or ten days according to the weather, and cover-glass preparations were made in the manner heretofore described. On March 17, 1887, a merino ram was procured for experimental purposes. This ram was from a flock that never had the disease, nor was there ever any of the disease on the farm. The feet were inspected by Dr. Detmers, myself, and others at the time of his arrival and all were perfectly normal. It was soon decided to inoculate one or more of the feet near the opening of the inter-ungulate gland, which was done in the right fore foot as follows: The foot was thoroughly washed by myself with warm water and soap, then with an aqueous solution of corrosive sublimate, 1 to 1000, then with alcohol and the latter allowed to evaporate.

By means of a small inoculating needle, disinfected in the flame of the spirit lamp, a small portion of gelatin culture, No. 4, was introduced just under the integument near the duct of the inter-ungulate gland. The ram was then kept in a clean, board-floor stall of the veterinary hospital, which never had been occupied by any sheep, and none had ever been in close proximity to it. Daily observations were made by myself and others; at first a little irritation and redness were present, probably due to the inoculating needle. In five days increased heat was quite perceptible, but it gradually subsided and nothing very abnormal could be observed except some fluid exhudation, or a little moisture. This condition of affairs continued, as daily observations were made by myself and others, until about the first of April. From that date appeared a rather

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foetid, whitish or yellowish discharge, which gradually increased until April 26, when it was pronounced genuine foot-rot by Drs. Townshend, Detmers, and all who saw it and were familiar with the disease. This condition of affairs continued only in a more aggravated form. The other feet remained normal, probably due to the fact that the ram was kept constantly in a dry place so that the exhudation dried before it came into a suitable nidus for its development. On April 29, a sterilized gelatin culture tube was inoculated with a portion of the discharge from the ram, with the platinum wire previously heated, and cultures made at intervals of a week or ten days. Numerous cover-glass preparations have been made of each of the cultures in the method already described.

Under the microscope with $\frac{1}{6}$ inch dry objective, Abbè condenser, Bulloch's biological stand, No. 9 eye-piece, and lamp-light, unstained cover-glass preparations in water showed numerous bacteria in motion, therefore they are motile.

At a superficial examination, especially in over-stained preparations, the bacteria might be mistaken for "oval micrococci" (Huppè, *Methods of Bacteriological Investigation*).

These presented themselves as single, short, rather angular rods, only a trifle longer than thick. They measured about 1.μ in length and about 0.7μ in thickness. There were double forms, two short rods joined by their ends, and also chains, of various lengths. These are no doubt true Bacilli. If we accept "oval Micrococci," and according to Huppè we must, these are somewhat different, having quadrangular parallel sides, and perhaps a little beveled corners. The joints are not the same length in different media. In different samples of gelatin a difference can be observed which could not happen if they were "oval Micrococci." They are also different in new and old cultures, those of a fresh

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culture being larger and much more resembling a typical Bacillus.

Under a $\frac{1}{15}$ homogeneous immersion objective of Tolles, N. A. 1.30, Abbè condenser, Beck's No. 1 eye-piece, tube length 238 mm., and lamp light, making an amplification of 845 diameters, the long chains showed to be bacilli without a doubt; a dark spot was also visible in each which was regarded as a nucleus. These nuclei were not to be seen with the dry $\frac{1}{6}$ objective and lamp light.

The size of the Bacilli were about 1.μ in length and about 0.7μ in width, which was determined in the following manner: A Zeiss glass rule, accurately divided into half millimeters, was held on the negative over the image of the Bacilli and they were found to be fully 1 mm. long and 0.7 mm. wide, and as the amplification of the negative was 1040, therefore the above results.

The vitality of the virus is a question of utmost importance to be determined. It will be seen that the virus survived the exposure of January weather from the time the thread was infected, until the gelatine tubes were inoculated in a dried condition, and kept in an envelope in a living room that was comfortable in the day time and cold at night. How much more exposure it would stand has not been determined, but the above was fully demonstrated. The cultures were discontinued in June, 1887, on account of the warm weather, but will be started again in the fall with a view to treatment, methods of prevention, etc.

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NOTE ON A MICROSCOPE PRESENTED BY LINNÆUS TO BERNARD JUSSIEU IN 1738.

By Jacob F. Henrici.

The microscope herewith presented for the inspection of the American Society of Microscopists was found in a lumber-room of the Harmony Society, a German community at Economy, Pennsylvania. It contains, in a drawer at the base of the stand, a Latin inscription, signed by Bernard Jussieu, setting forth that he received the instrument from his very dear friend Linnaeus, as a gift of friendship, in lasting memory of the pleasant intercourse which they had at Paris in the month of August, 1738.

The microscope is said by the present aged members of the Harmony Society to have belonged formerly to Frederick Rapp, one of the founders of the Society, who came to America from Germany in 1804, and who died at Economy in 1834. He was a man of considerable

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culture, and much of the prosperity of the community was due to his intellectual activity. No one now knows when or how the instrument came into his possession, or what use he made of it.

The body of the microscope is of pasteboard, or *papier-mâché*, with wooden mountings, and fixed vertically on a wooden stand. It is provided with a draw-tube, and the adjustment is by means of a screw. Ten objectives accompany the instrument, each consisting of a single lens, ranging in focal distance from about one-quarter of an inch to an inch. The lenses range in diameter from six millimetres to a centimetre; but when in position they are stopped down by brass caps to an aperture of about two millimetres diameter. Unfortunately one of the lenses of the eye-piece is lacking, and in order to exhibit the power of the instrument, I have replaced it for the moment by a corresponding lens from my working microscope. No maker's name appears on any part of the instrument.

The inscription, in full, is as follows:

Audax Iapeti genus
Ignem fraude malâ gentibus intulit
Nil mortalibus arduum.
Hor. Carm. Lib. I. 3.
In perpetuam memoriam
consuetudinis quam cum
dulcissimo suo sodali
Carolus Linne Parisiis
habebat hoc ab eo amicitiae
donum accepit, mense
Augusto, MDCCXXXVIII
Bernardus Jussieu.

Aside from the interest attaching to this microscope from its association with two of the great scientific workers of the last century, it is encouraging to compare our microscopes of to-day with this crude instrument, which Jussieu deemed worthy of the admiration expressed in Horace's line: "Nil mortalibus arduum."

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THE COMPARATIVE SIZE OF BLOOD CORPUSCLES OF MAN AND DOMESTIC ANIMALS.

By Freda Detmers, B. Sc., Ohio State University, Columbus, Ohio.

Ever since the Hayden murder trial (October, 1879), in Connecticut, the subject of the comparative size of the blood corpuscles of man and animals has awakened greater interest in the minds of microscopists and medico-legal experts than it had before, and by many it has been made a special work.

The point of difference about which discussion centers itself is, that some investigators attribute to the microscope the power to distinguish between the blood corpuscles of human beings and those of other animals, especially of the dog. There are others who take the opposite ground, and say that no such distinction can be made.

That the investigation is of great importance, must, I think, be admitted by all; as yet, however, no definite results have been obtained. Thus far each one has had to rely on his personal experience and investigation into the subject.

With a view to ascertain whether there is, or is not an essential difference as to size and outline between the blood corpuscles of man and some of our domesticated animals, I determined to investigate the subject for myself.

In order that my measurements could in no way be influenced by the results obtained by others, the reports published from time to time were not studied until all my measurements were completed.

Of the ten specimens of blood examined, all were taken from live animals, with the exception of that of the new-born rabbit, which had been dead but a few hours. The

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cat was under the influence of chloroform when the drop of blood was taken, and the blood of the new-born child was taken from the umbilical cord, and was obtained through the kindness of a physician.

All the slides were prepared under the same circumstances and in exactly the same way, viz: by putting a small drop of blood on a perfectly clean slide, near the edge, then passing this slide, drop downwards, gently over another perfectly clean slide, and thus spreading the drop of blood over the surface of the latter in a uniformly thin layer. When this was dry a ring of shellac was run on the slide, and then a perfectly clean cover glass, 90μ in thickness, was carefully placed on the ring, and when this was dry, a ring of shellac was run around the cover.

The instruments used in the examination of the blood are Bulloch's professional stand with mechanical stage; Tolles' $\frac{1}{15}$ homogeneous immersion objective, corrected at 1; numerical aperture, 1.30; Bulloch's No. 9 eye-piece, with cobweb micrometer; W. A. Rogers' stage micrometer, divided in 1-100 millimeters (10 mm. or 1001 lines), and in 1-2500 inches (2-5 inches, or 1001 lines); an Abbé condenser made by Bulloch, and used with a small diaphragm; a common coal oil lamp.

In all the measurements the objective was corrected at 1; cedar oil was used as immersion fluid, and the same length of tube, 238 mm., as also the same *optical* tube length was maintained.

It is true that with the high amplification I used — 1,700 diameters — no objective, however good, brings out the outline of the corpuscles as sharp as a mathematical line. Jugdment must be used as to how much of this so-called shadow surrounding the corpuscles shall be included in the measurement of the corpuscle, and how much shall be regarded as shadow. Then, again, the cobwebs of the micrometer have a certain thickness. This is, however, a much more accurate means of determining the actual size of a corpuscle than by

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using the glass micrometer, and then guessing at the exact size, where it cannot be measured.

On each side, the corpuscles, which were approximately in the centre of the field, as the slide was moved from right to left, were measured. Those that were higher or lower than about one-fourth the diameter of the field were not measured, because the focus of the outline of these corpuscles above or below a certain distance from the center is never fully as sharp as of those in the centre, and in bringing them to the stationary cobweb, and in a position where they can best be seen, the slide must needs be moved up and down, as well as sideways. In doing this, confusion would arise as to which had been, and which had not been measured. Of the corpuscles in the centre of the field, only those were measured which appeared to be approximatively round; those showing a marked irregularity in shape were skipped.

One hundred corpuscles on each slide were measured, the measurements taken in divisions of the eye-piece micrometer. Each measurement, as soon as read off, was recorded in the tables accompanying this paper.

Only the average size of the whole 100, and that of the largest and smallest corpuscles, were reduced from these divisions to mikra. 21.16 of these divisions, with the optical apparatus employed, are equal to one mikron. This is the result of many measurements of Rogers' stage micrometer.

The measurements were not reduced to inches, because the mikron is accepted by the American Society of Microscopists as the *standard* microscopical *unit*, and as such it is also recognized all over Europe.

The photographs were taken so as to include as many as possible of the corpuscles that were measured. These can be measured on the negative, and by this means the first results can be verified.

In making the photographs, I used a Blair camera, Bulloch's professional stand, Tolles' $\frac{1}{15}$ homogeneous immersion objective, corrected at 1; Beck's No. 1 eye-piece

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(approximately a 2-inch eye-piece); an Abbé condenser, made by Bulloch, used with a small diaphragm; a large Bull's eye-condenser, made by Bausch & Lomb; a coal oil lamp with a flat No. 3 burner; Seed's 5x7 dry plates, sensitometer 21. Time of exposure, 8 minutes. Exactly the same length of tube, and the same length of camera was used for all the photographs, and as Rogers' stage micrometer was also photographed with precisely the same appliances, the exact amplification, which is 1040 diameters, could be easily measured on the negative.

After carefully examining the specimens of blood, I feel that I can assert without fear of contradiction, that there can be no question but that the blood of human beings can readily be distinguished from that of such animals as the mule, cat, calf, and horse, and more readily from cattle, sheep, and pigs. As there is so marked a difference, I have thought it necessary to examine only the blood of the first three, and to record the results obtained.

The blood corpuscles of these animals are not only on the average much smaller, but the largest corpuscles measured are smaller than those of an average size of the human blood. This can easily be seen by referring to the tables of measurements, and also to the photographs.

Between the blood of the rabbit, dog, and man, finer discrimination must be made. Although the average size of the blood corpuscles of the two former is smaller than that of man, the difference is not very great. It may be, however, that by further investigation, by examining other specimens of the blood of these animals, this difference may be found to be a permanent one, and again we may find it greatly diminished. A much more thorough and extensive examination than I have been able to make here, must be made before a difference can, or ought to, be established with certainty.

It is a much simpler matter to distinguish between the blood corpuscles of a very young and full-grown animal of

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the same species than it is to distinguish between those of the full-grown rabbit, dog, and man. The blood corpuscles of very young animals lack symmetry of outline and show a marked inequality of contour. Only the minority of the corpuscles are approximately round, and these round ones are chiefly those which are more or less isolated on the slide.

A noticeable feature in this connection, discovered in these examinations, is that almost invariably the largest corpuscles are found where the corpuscles are scattered; and in a field where they are numerous and close together the general average also is smaller.

The greater size of the blood corpuscles of young animals, their want of symmetry in outline and want of roundness can all be explained, if we assume that they are less dense, and hence softer than those of older animals. And it seems that the manner in which the slides were made, brought to bear a greater pressure on these softer corpuscles than they could resist, and hence they became spread out and show distortions which are rarely found in the blood corpuscles of older animals, which we must assume have become more dense and compact as the animal grew older.

In the specimens examined of the blood of the old dog and of the full-grown person, there is certainly a difference in size as to the average, and also as to the largest and smallest corpuscles. So, if in, suppose, a criminal case, it should be claimed that a certain specimen of blood is that of an old dog, it might be determined without much diffculty whether it was human or dogs' blood. But suppose it was claimed to be blood of a young dog, one still entitled to be called a pup, then it would be almost impossible to determine which it was, if one expected to decide the question by the difference in size only.

For although there is a very marked difference in the sizes of the corpuscles of a young and an old dog, the size of the corpuscles of a young dog correspond pretty closely with those of a grown person.

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Thus, as to size, we might be at a loss to say which it was, but a decision might be arrived at through further investigation, as by employing other means than those of measurement only; here the difference in contour and apparent density would be an important point.

The largest corpuscle, 12+μ, found in the specimen of child's blood, is very much larger than any yet found, but the average of the 100 corpuscles measured is smaller (to however so slight an amount that it may be due to some mistake in the measurements) than the average of the young dogs' blood. I would, therefore, pronounce it impossible, on the strength of the measurement made, to distinguish between the young child's and the puppy's blood.

But even with all the data here furnished, to look at the matter carefully from all sides, it would indeed be rash, and in many cases criminal, to arrive at a final conclusion. The investigator must be expert, exact, and must have had experience in this work, and not only that, but he must be conscientious, as well. It will not do to take for granted that the average size of the entire quantity of blood corpuscles in any one animal, and much less the average size of the whole quantity in all the animals of one species, corresponds exactly to the average size of the one hundred corpuscles taken from a single drop of blood.

Of course it is utterly impossible to measure every corpuscle in the blood of an animal, but a thousand or more should at least be measured before the mere hypothesis, that there is a well defined difference between the blood corpuscles of human beings and those of some of our domesticated animals, can be established as a fact.

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HUMAN BLOOD.— No. 1.

No. of corpuscle.	Spaces. 21.16 sp.= 1μ.	No. of corpuscle.	Spaces. 21.16 sp.= 1μ.	No. of corpuscle.	Spaces. 21.19 sp.= 1μ.	No. of corpuscle.	Spaces. 21.16 sp.= 1μ.
1	171	26	185½	51	199	76	172
2	167	27	177	52	177	77	177
3	188	28	194	53	172	78	146
4	177	29	180	54	179	79	179
5	179	30	176	55	175	80	168
6	174	31	182	56	170	81	175
7	184	32	156	57	162	82	180
8	152	33	154	58	176	83	172
9	193	34	175	59	173	84	185
10	175	35	188	60	171	85	180
11	**201**	36	177	61	173	86	184
12	179	37	164	62	183	87	169
13	174	38	195	63	170	88	178
14	177	39	180	64	163	89	176
15	174	40	178	65	182	90	170
16	189	41	171	66	169	91	181
17	175	42	167	67	177	92	160
18	170	43	176	68	167	93	176
19	178	44	172	69	191	94	190
20	181	45	155	70	179	95	173
21	186	46	164	71	**141**	96	166
22	186	47	161	72	168	97	194
23	177	48	190	73	155	98	182
24	167	49	171	74	200	99	162
25	184	50	174	75	163	100	189

The largest corpuscle measures 201 spaces = $9^{499}\mu$.

The smallest corpuscle measures 141 spaces = $6^{6635}\mu$.

The average of 100 corpuscles 175^{395} spaces = $8^{282}\mu$.

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HUMAN BLOOD.—No. 2.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	171	26	175	51	168	76	185
2	169	27	164	52	164	77	172
3	182	28	168	53	163	78	149
4	167	29	161	54	172	79	170
5	156	30	169	55	191	80	176
6	174	31	165	56	164	81	163
7	162	32	162	57	182	82	171
8	189	33	167	58	175	83	181
9	177	34	166	59	186	84	173
10	163	35	182	60	172	85	164
11	174	36	168	61	163	86	154
12	180	37	162	62	180	87	159
13	169	38	172	63	160	88	173
14	154	39	192	64	176	89	182
15	175	40	153	65	163	90	171
16	165	41	160	66	178	91	**146**
17	170	42	165	67	179	92	166
18	162	43	172	68	165	93	164
19	158	44	166	69	150	94	169
20	181	45	163	70	157	95	157
21	**194**	46	175	71	168	96	172
22	179	47	164	72	177	97	165
23	166	48	174	73	166	98	174
24	163	49	149	74	161	99	192
25	168	50	175	75	168	100	170

The largest corpuscle measures 194 spaces = $9^{1682}/_{\mu}$.
The smallest corpuscle measures 146 spaces = $6^{8998}/_{\mu}$.
The average of 100 corpuscles 169^{13} spaces = $7^{9922}/_{\mu}$.

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BLOOD OF NEW-BORN WHITE RABBIT.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	175	26	167	51	181	76	179
2	164	27	170	52	189	77	171
3	171	28	171	53	171	78	186
4	159	29	185	54	200	79	189
5	167	30	193	55	185	80	163
6	178	31	191	56	128	81	165
7	164	32	174	57	191	82	168
8	161	33	141	58	189	83	158
9	175	34	185	59	115	84	186
10	171	35	188	60	175	85	191
11	194	36	192	61	191	86	176
12	197	37	174	62	186	87	159
13	184	38	189	63	196	88	182
14	173	39	170	64	198	89	189
15	181	40	173	65	170	90	185
16	202	41	178	66	165	91	153
17	**89**	42	177	67	192	92	189
18	167	43	156	68	179	93	186
19	146	44	180	69	189	94	168
20	211	45	185	70	186	95	175
21	184	46	167	71	173	96	166
22	178	47	181	72	201	97	181
23	117	48	170	73	175	98	182
24	177	49	168	74	160	99	176
25	**214**	50	138	75	196	100	174

The largest corpuscle measures 214 spaces = $10^{11\frac{84}{100}}\mu$.

The smallest corpuscle measures 89 spaces = $4^{2\frac{06}{100}}\mu$.

The average of 100 corpuscles $176\frac{4}{100}$ spaces = $8^{3\frac{36}{100}}\mu$.

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BLOOD OF AN AGED RABBIT.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	162	26	166	51	158	76	154
2	148	27	154	52	157	77	171
3	167	28	152	53	154	78	158
4	162	29	156	54	161	79	160
5	169	30	148	55	154	80	161
6	**178**	31	155	56	150	81	153
7	152	32	171	57	160	82	146
8	174	33	153	58	157	83	154
9	155	34	154	59	161	84	145
10	143	35	160	60	154	85	161
11	157	36	143	61	160	86	153
12	172	37	161	62	157	87	158
13	170	38	165	63	145	88	146
14	144	39	158	64	156	89	154
15	147	40	167	65	170	90	**112**
16	164	41	154	66	162	91	138
17	161	42	152	67	172	92	154
18	154	43	155	68	159	93	170
19	163	44	152	69	148	94	153
20	159	45	163	70	158	95	164
21	142	46	156	71	159	96	161
22	168	47	159	72	165	97	158
23	157	48	150	73	168	98	165
24	152	49	147	74	154	99	155
25	160	50	152	75	157	100	152

The largest corpuscle measures 178 spaces = $8^{41209}\mu$.
The smallest corpuscle measures 112 spaces = $5^{293}\mu$.
The average of 100 corpuscles 156^{93} spaces = $7^{41\frac{68}{100}}\mu$.

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BLOOD OF DOG.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	**196**	26	175	51	155	76	180
2	169	27	164	52	148	77	164
3	159	38	183	53	168	78	155
4	157	29	162	54	167	79	165
5	164	30	168	55	153	80	178
6	175	31	163	56	160	81	167
7	163	32	173	57	184	82	153
8	155	33	186	58	160	83	143
9	158	34	170	59	163	84	**138**
10	171	35	169	60	169	85	156
11	143	36	139	61	153	86	153
12	150	37	160	62	150	87	165
13	142	38	152	63	155	88	161
14	171	39	161	64	161	89	163
15	164	40	167	65	159	90	165
16	166	41	141	66	164	91	171
17	155	42	175	67	158	92	158
18	165	43	156	68	186	93	174
19	167	44	150	69	163	94	170
20	174	45	185	70	166	95	165
21	157	46	161	71	175	96	164
22	162	47	167	72	170	97	167
23	151	48	152	73	163	98	159
24	176	49	155	74	152	99	154
25	163	50	138	75	166	100	164

The largest corpuscle measures 196 spaces = $9^{26\,27}\mu$.

The smallest corpuscle measures 138 spaces = $6^{52\,17}\mu$.

The average of 100 corpuscles 162^{65} spaces = $7^{67\,24}\mu$.

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BLOOD OF PUPPY FIVE DAYS OLD.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	205	26	193	51	174	76	182
2	209	27	214	52	210	77	198
3	197	28	186	53	193	78	202
4	178	29	182	54	194	79	179
5	186	30	**220**	55	199	80	195
6	196	31	199	56	167	81	184
7	207	32	194	57	196	82	190
8	188	33	195	58	212	83	175
9	196	34	190	59	202	84	**160**
10	191	35	204	60	183	85	191
11	189	36	179	61	176	86	186
12	217	37	191	62	188	87	183
13	195	38	205	63	193	88	195
14	182	39	187	64	180	89	176
15	170	40	197	65	185	90	217
16	210	41	188	66	200	91	188
17	177	42	172	67	208	92	184
18	179	43	191	68	183	93	169
19	207	44	188	69	199	94	178
20	209	45	167	70	191	65	181
21	184	46	181	71	193	96	213
22	181	47	171	72	189	97	209
23	174	48	176	73	177	98	197
24	202	49	210	74	179	99	203
25	182	50	205	75	196	100	196

The largest corpuscle measures 220 spaces = $10\frac{397}{\mu}$.

The smallest corpuscle measures 160 spaces = $7\frac{5614}{\mu}$.

The average of 100 corpuscles 191^{04} spaces = $9\frac{0283}{\mu}$.

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BLOOD OF MULE.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	128	26	122	51	140	76	123
2	115	27	138	52	131	77	146
3	137	28	129	53	149	78	136
4	122	29	125	54	113	79	120
5	132	30	127	55	131	80	145
6	121	31	126	56	118	81	112
7	118	32	128	57	124	82	145
8	131	33	145	58	135	83	125
9	118	34	**97**	59	131	84	111
10	145	35	120	60	128	85	134
11	110	36	132	61	120	86	139
12	123	37	128	62	112	87	119
13	127	38	119	63	136	88	121
14	130	39	140	64	123	89	123
15	135	40	137	65	130	90	136
16	129	41	121	66	144	91	135
17	120	42	127	67	113	92	116
18	130	43	143	68	112	93	134
19	134	44	158	69	115	94	133
20	123	45	132	70	125	95	132
21	115	46	127	71	116	96	124
22	125	47	130	72	132	97	135
23	111	48	120	73	134	98	138
24	148	49	131	74	126	99	132
25	124	50	135	75	115	100	**165**

The largest corpuscle measures 165 spaces = $7\frac{7977}{10000}\mu$.
The smallest corpuscle measures 97 spaces = $4\frac{5841}{10000}\mu$.
The average of 100 corpuscles 128^{8} spaces = $6\frac{0688}{10000}\mu$.

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BLOOD OF CALF THREE DAYS OLD.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	121	26	137	51	125	76	137
2	133	27	134	52	117	77	123
3	123	28	128	53	138	78	117
4	140	29	125	54	125	79	123
5	130	30	122	55	122	80	128
6	132	31	125	56	131	81	125
7	125	32	117	57	127	82	101
8	110	33	133	58	115	83	123
9	121	34	120	59	127	84	121
10	123	35	116	60	139	85	126
11	137	36	121	61	114	86	117
12	116	37	126	62	129	87	126
13	115	38	114	63	124	88	127
14	119	39	131	64	123	89	119
15	131	40	115	65	137	90	102
16	122	41	133	66	142	91	**148**
17	100	42	132	67	129	92	116
18	140	43	139	68	117	93	122
19	116	44	136	69	**90**	94	138
20	126	45	119	70	124	95	125
21	119	46	120	71	132	96	113
22	118	47	125	72	128	97	128
23	120	48	132	73	123	98	110
24	131	49	121	74	130	99	123
25	127	50	133	75	119	100	119

The largest corpuscle measures 143 spaces = $6\frac{758}{1000}\mu$.
The smallest corpuscle measures 90 spaces = $4\frac{253}{1000}\mu$.
The average of 100 corpuscles $124\frac{8}{100}$ spaces = $5\frac{874}{1000}\mu$.

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BLOOD OF AN AGED CAT (MALE).

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	130	26	114	51	117	76	138
2	134	27	132	52	114	77	123
3	123	28	116	53	125	78	144
4	130	29	149	54	128	79	132
5	128	30	119	55	122	80	126
6	115	31	114	56	120	81	117
7	108	32	115	57	118	82	119
8	133	33	119	58	135	83	135
9	141	34	136	59	120	84	124
10	128	35	113	60	141	85	114
11	124	36	134	61	143	86	123
12	122	37	122	62	**150**	87	129
13	129	38	136	63	132	88	125
14	110	39	111	64	123	89	126
15	131	40	132	65	119	90	140
16	125	41	130	66	121	91	138
17	118	42	141	67	127	92	117
18	146	43	116	68	122	93	137
19	125	44	126	69	116	94	132
20	143	45	136	70	129	95	**103**
21	124	46	123	71	147	96	144
22	113	47	114	72	125	97	126
23	144	48	121	73	125	98	139
24	127	49	135	74	128	99	122
25	147	50	132	75	108	100	124

The largest corpuscle measured 150 spaces = $7^{0888}/\mu$.
The smallest corpuscle measures 103 spaces = $4^{8676}/\mu$.
The average of 100 corpuscles 126^{85} spaces = $5^{9948}/\mu$.

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BLOOD OF NEW-BORN CHILD.

No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.	No. of corpuscle.	Spaces. 21.16 sp. = 1μ.
1	**260**	26	196	51	199	76	185
2	181	27	223	52	186	77	166
3	194	28	186	53	191	78	194
4	186	29	171	54	217	79	213
5	198	30	180	55	149	80	187
6	205	31	224	56	174	81	177
7	209	32	183	57	194	82	175
8	188	33	207	58	208	83	159
9	185	34	219	59	209	84	184
10	175	35	177	60	185	85	212
11	176	36	212	61	190	86	211
12	200	37	184	62	195	87	190
13	191	38	193	63	193	88	162
14	182	39	178	64	176	89	196
15	215	40	248	65	191	90	210
16	187	41	225	66	178	91	196
17	207	42	189	67	181	92	205
18	189	43	198	68	159	93	187
19	160	44	182	69	167	94	188
20	208	45	218	70	215	95	192
21	213	46	196	71	169	96	181
22	**140**	47	224	72	201	97	162
23	183	48	163	73	173	98	195
24	201	49	167	74	196	99	191
25	205	50	160	75	158	100	180

The largest corpuscle measures 260 spaces = $12\frac{2873}{}\mu$.

The smallest corpuscle measures 140 spaces = $6\frac{6162}{}\mu$.

The average of 100 corpuscles $191\frac{03}{}$ spaces = $9\frac{0288}{}\mu$.

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EXPLANATION OF THE PLATE.

I.— Blood of aged persons (54 years), showing one of the largest blood corpuscles measured on this slide.

II.— Blood of same person — showing average size of corpuscles.

III.— Blood of full-grown young person — showing average size of corpuscles.

IV.— Blood of new-born white rabbit.

V.— Blood of aged female rabbit, mother of the new-born rabbit, of which sample represented by Plate IV was taken.

VI.— Blood of middle-aged full-grown dog — common street dog, medium size.

VII.— Blood of puppy, five days old.

VIII.— Blood of mule — showing near the centre the largest corpuscle found on the slide.

IX.— Blood of Jersey calf, three days old.

X.— Blood of aged cat.

XI.— Blood of new-born child — showing the largest corpuscle found on the slide. The sample of blood was taken from the umblical cord.

XII.— Blood of same child, also from the umblical cord, showing a field of nearly round corpuscles of about average size.

NOTE.— The illustrations herewith presented are process reproductions from the original photographic negatives, hence must exactly correspond with the photo-prints. —[ED.

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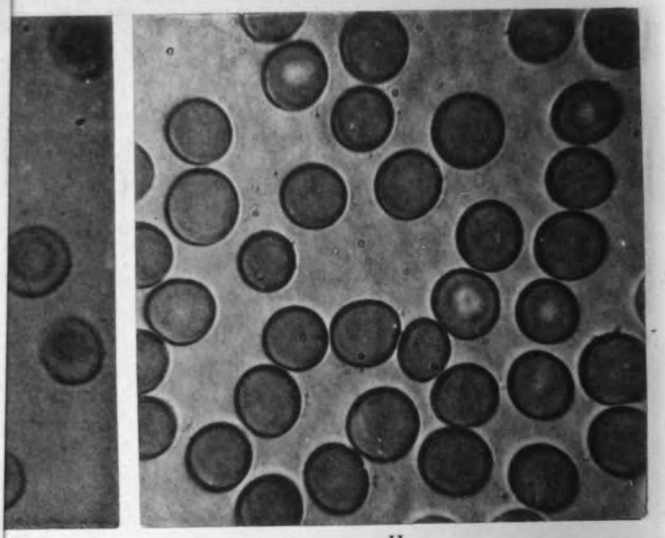
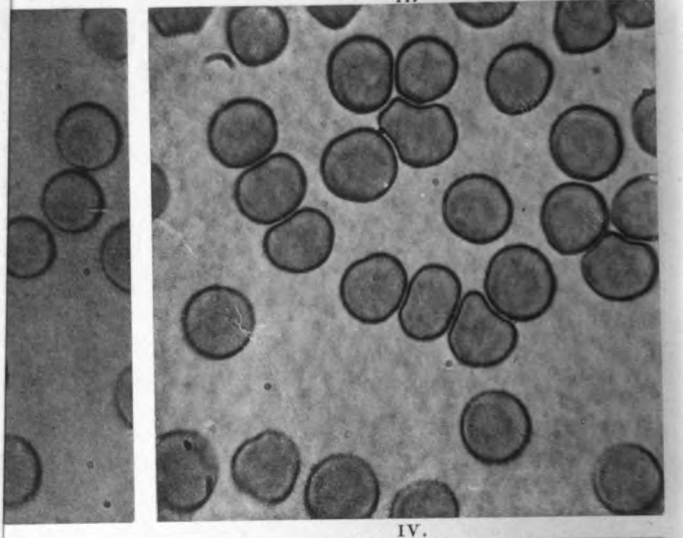
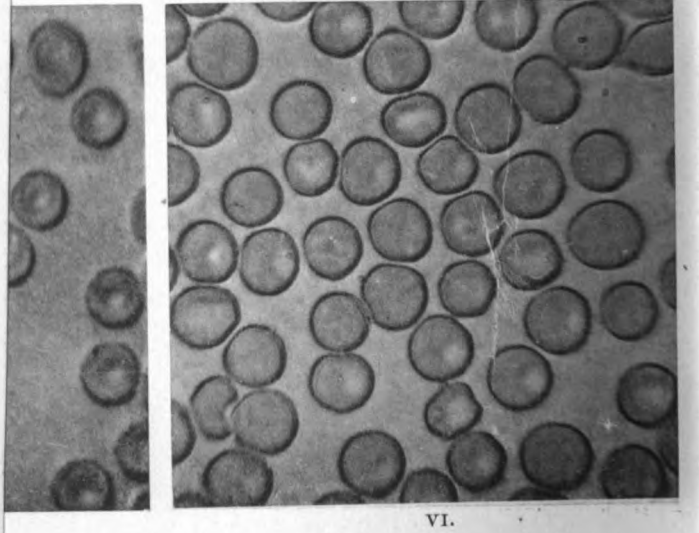


II.



IV.



VI.

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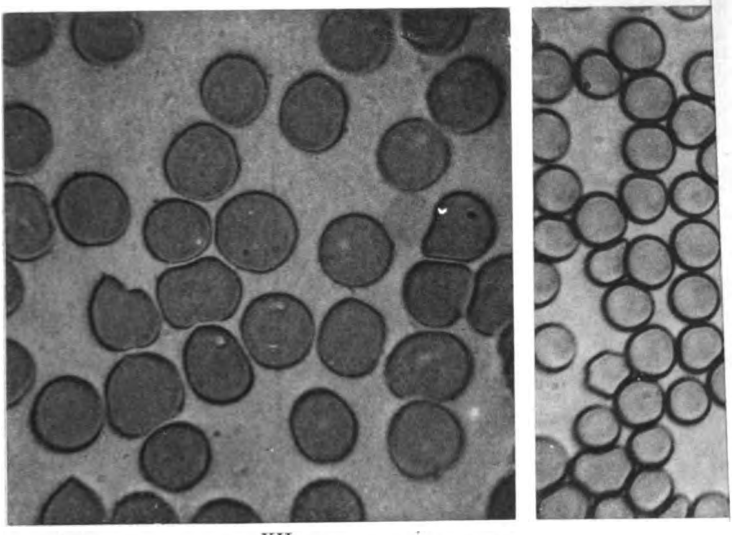
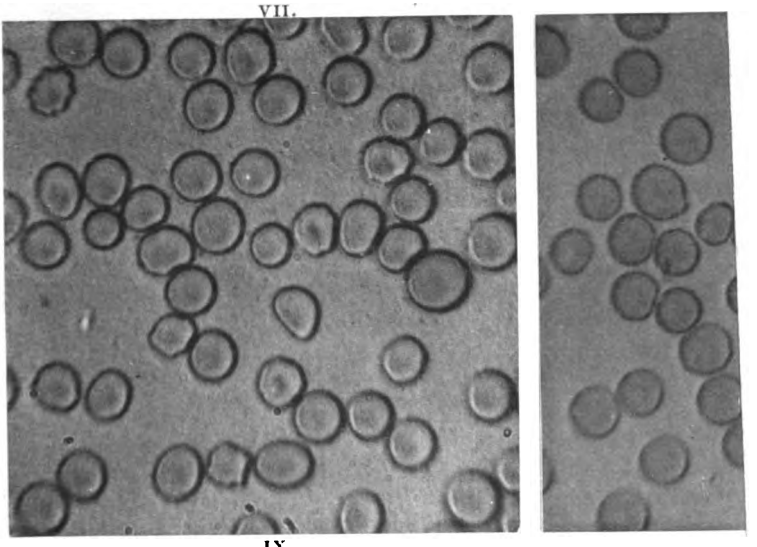
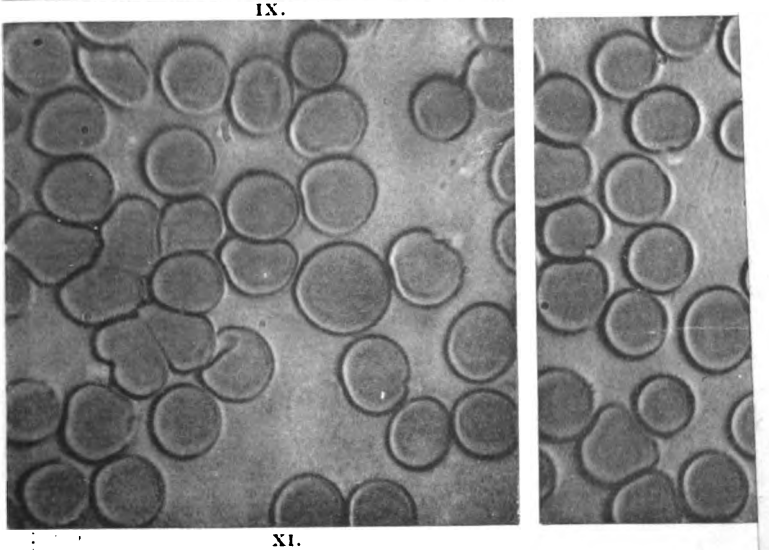


VII.



IX.



XI.

Chicago Art Publishing

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ON A MICROSCOPICAL SLIDE-CATALOGUE.

By R. H. Ward, M. D., F. R. M. S., Troy, N. Y.

The system of cataloguing histological and other microscopical preparations here shown is primarily intended as a substitute for the card-catalogue, for the use of that very large and important class of workers who do not, need not, and will not, undertake the labor of maintaining the more cumbersome and troublesome system by cards; but it is also believed to be available in connection with cards for those who may find it necessary to employ also the latter.

There is a great deal of useful work done with the microscope, and more that ought to be useful, along the various lines of investigation among plants and animals, by students who use the microscope so little and so incidentally, that they attach slight importance to the work, and make no formal effort to preserve it either in mounts or in manuscript. But from time to time, they casually lay aside a slide which contains some important object, or illustrates some special point which is likely to be required for future use. The exact character and history of each slide, and the various points for which it is valued, are so fresh in the mind, that they are assumed to be a part of the possessor's permanent resources, and no question is raised about them. But as years pass by, the few dozens of slides become hundreds and then thousands, the recollections of them become distant and dim, and crowded by more recent thoughts, the precise peculiarities of preparation, or the characteristics for which they were wanted are forgotten, and are necessarily

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very imperfectly represented upon the labels. A written sheet or card descriptive of each slide might have been carefully prepared and laid aside where it could be found when wanted, but it wasn't, and probably wouldn't have been had the resolution to do so been formed ever so many times; and the owner at last begins to realize that his collection was both made and marred before he became aware that he was forming one at all. A suitable history of his cabinet would be invaluable, or at least measured only by the possible value of the collection itself; for without it most of the slides are little better than rubbish, and considerable trouble might have been saved by throwing them away to begin with. The point cannot be too well remembered that any slide which is worth preserving at all, for any scientific purpose, is worthy of a proper registry at the time of acquiring it; not by merely numbering it and entering its name in one line of a book, the line afterwards growing to two or three as crowding ensues and experience shows more space to be necessary, but by cataloguing in a descriptive manner according to some sensible and thoroughly systematic plan which is likely to be permanently satisfactory. When, if ever, the beginner becomes a specialist, with trained assistants, and the resources of a laboratory with its card-catalogue at command, then such a register may still continue useful as a serial list and for the temporary preservation of data concerning objects in course of preparation.

If there were only two or three things to be recorded of a slide, they might be written in a fixed order by mere force of habit without further formality, like a library catalogue-card. But the really valuable data concerning a prepared slide are too numerous to be easily selected and arranged by memory alone, the desired thoroughness and uniformity of description requiring some artificial assistance. By far the simplest and best method would be to tabulate the data in columns down a page, but the items are too numerous to admit of this arrangement on a page of available size, and

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much convenience would be lost by sub-dividing, so that all could not be seen at once. But by giving three or four lines to each entry, conspicuously mapped out by red lines, and entering a like number of related items in each column, the desired end can be easily and fully attained. By adopting the ordinary quarto size of page, assigning ten numbers to each page (the decimal arrangement being most convenient for rapid reference), and carrying the entries acrost each pair of facing pages as seen when the book is opened, an area about equal to that of a postal card can be assigned to each object, this being an adequate amount for most purposes of record, and the most that can be secured without sacrifice of convenience as a catalogue. Of course numerous contractions for familiar names of apparatus, processes, etc., will inevitably be used to prevent crowding; and occasional long historical or critical notes, or frequently recurring formulæ will be entered in an appendix of separate blank leaves, and referred to by number.

The items most commonly desired are tabulated in print at the head of each page, and only referred to by suggestive letters below; so that the description of each slide appears as a practically uninterrupted block of manuscript, easy to read and available for substitution of other data according to individual necessities. The printed letters down the page become so familiar after a few minutes' use as to be scarcely thought of afterwards, they serving merely as hints to indicate where to enter certain data in a blank page, or where to look for them in a filled one. Certain items are entered during the preparation of a slide, or when it is laid aside in the cabinet, and others, especially cross-references to various points of structure, or references to books or journals, may be suggested and entered from time to time, as the slide is used or the literature of the subject studied; so that, with scarcely appreciable labor, the educational value of one's cabinet is constantly enhanced.

As arranged by the writer, and shown in the accompany-

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ing plate, the data are grouped in four columns, the first giving the common and scientific *names* of the object and its source, and the second its *habitat* or locality, collector, etc. Beneath these is room for a long entry as to *special points* shown, references, criticisms, or other notes not classified elsewhere. The third column gives the *preparation* of the object, how preserved, hardened, softened, injected, etc., how manipulated or dissociated, cut, teased, etc.; and the staining and clearing media, if any. This column, as lettered, is almost equally suited to histological or pathological work, either vegetable or animal; but by introducing obvious substitutions it may be made available for slides in lithology, micro-chemistry, etc., and thus the catalogue becomes available for a mixed general collection. The fourth column gives details of *mounting*, the medium, cell and cements, cover-glass, date, location in cabinet, repairs, etc.

The thickness of cover-glass should always be entered if exceptionally thick or thin, for obvious reasons; and in any case it should be entered if measured, to obviate the possible necessity for measuring it again, whether in reference to the selection of a suitable objective, or for the approximate adjustment of the screw-collar, or for saving time, when examining many preparations, by taking those in succession which will require little, if any, change of adjustment. Whether measured by the special instruments sold for the purpose, or by the microscope micrometer, the cover being held edgewise in the stage forceps, or in a notched cork supported upon the stage (the most accurate but most troublesome way), or, after mounting, by focussing with the fine adjustment wheel, first upon the upper and then upon the lower surface of the cover, or on the top of the object in some balsam mounts, and recording the difference between the two readings, it should of course be done always in exactly the same manner. The latter method (with or without an estimated correction for refraction, so as to give the real

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instead of the apparent thickness of the glass) is sufficiently accurate and is perhaps the best way, as it can be done equally well at any time and is the only method by which neglect at the moment of mounting can be satisfactorily corrected at convenience thereafter. For slides used frequently in teaching, it often saves time to record also the approximate cover-adjustment of the lenses with which they are commonly employed, the final correction being given, of course, at the moment when required.

The location in cabinet must be specified in large classified collections, to save time in finding slides whose character might allow them to be placed equally well in any one of the four or five different places. In some large collections these unavoidable complications have led to the abandonment of a natural classification and the arrangement according to the serial numbers, each drawer being devoted to a certain regular succession of numbers; by which means any slide can be promptly found when wanted, but the great convenience of having similar objects (as diatoms, algæ, or seeds) grouped together for comparison and selection is lost. By this system of cataloguing, however, the slides may be classified in any way most convenient to the owner and still be found as readily as in the awkward serial arrangement.

The Alphabetical Index is, of course, a large and an essential portion of this system. Its pages are specially ruled for convenience in entering titles and numbers, and they have a capacity for several references to each slide. The volume for 2,000 slides has room for nearly 10,000 references, or about five for each slide. Thus a leaf preparation may not unlikely be referred to under both popular and scientific names of the plant and also under several such titles as "Leaf of ——", "Spiral Vessels in ——", "Stomates of ——", "Hairs of ——", "Starch in ——", "Raphides in ——", etc. But as many simple slides require only two or three entries, the more complex ones will have space for eight or ten.

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The Index is lettered alphabetically, the number of pages assigned to each letter depending upon the frequency with which that letter occurs at the beginning of English words. Subdivision is accomplished according to the vowel system of arrangement, whose advantages are familiar to all readers, and which may, by means of a few obvious expedients, be made applicable to slide-catalogues of various sizes. Thus the pages devoted to any letter, as **s**, are divided into six portions and lettered **sa, se, si, so, su, sy**; the first portion being for words beginning with **s** and having **a** for their first vowel, and so on for the rest. Further subdivision is not specified in the forms, because it depends so largely upon individual wants as to be best left optional with the user. But having given a page to the **sa** words, for instance, it is difficult to believe that any thoughtful person could possibly throw all these together at random. Probably nearly every one would enter things pertaining to animals at the top of the page, vegetables in the middle and minerals at the bottom, or *vice versa*. A specialist in any department would give the lion's share of the page to his particular province, subdivided to suit himself; and the vegetable kingdom, being in the middle, could be carried up or down, where experience showed, as the page gradually filled, that room could best be spared. After such entries as starch, pollen, hair, etc., several lines would be left blank for similar items, so that ultimately these items would appear in blocks that would be instantly recognized on glancing at a page. In larger collections where **sa** included many pages, a number of these whole pages would be assigned to animal, vegetable and mineral objects respectively. In this case a botanist, for instance, would probably reserve more pages for plants than for all the rest; and he might at first devote a column or even a whole page for such a group as starches, and a like portion of **se** for seeds, one column of the seed page being given to whole seeds and another to sections, etc. Subsequently, if too much space proved to have been reserved,

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the lower portions of the vacant parts would be filled with other things. By such expedients, a rough but most useful working classification of the pages and of their contents can be maintained until the book is nearly full.

The accompanying sample page (*page 240*) of **sa** entries of familiar objects, though much more crowded and therefore less satisfactory than in actual use, shows how such a plan is carried out, and with what facility any object may be found in a collection of 3,000 or 4,000 slides.

Obviously the catch word by which an entry will be found is its first word, by which it was located and sought for; and the other most characteristic word, which distinguishes the item from others of its kind, and which may or may not be the only other word, may be underlined for easy recognition. The writer uses pencils of different colors for this purpose, in the serial list as well as in the index, red for animal, green for vegetable and blue for mineral specimens, and thus gains a perspicuity whose value is evident. By a little extra care in labeling the slides, the same distinction of colors may be extended to the labels, using red, green and blue tinted papers, or white paper with borders printed in those colors, as a means for readily recognizing and distributing the slides themselves whenever they have become mixed in use.

Though not admitting the absolutely alphabetical sequence attained by cards, this system is in some respects even more practical than that for small collections, say up to three or four thousand slides. It is easier to see and compare numerous items when collated upon a page than when stacked away in cards. Thus fifty or sixty entries of hairs or of crystals can be reviewed and compared, and a half-dozen selected for some purpose, much better by glancing over a page than by leafing over that number of separate cards; while the graphic effect of the page is of perceptible use in keeping one's mind constantly familiar with the extent and character of his collection. The cards are theoretically

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SA. (*Animal.*)	*No.*
Saw-Fish, Tooth, Sec....	233
Scaly Epithelium.....	272
Scale Insect.....	2440–3, 2364
Scales (See Wings).	
" Mosquito.....	273
" Lepisma	2066
" Podura.....	2797, 2090
" Cabbage But'fly..	2106
" Tinea	2699
" Sole, and *in situ*..	665–6
" Trout	1596
" Flound'r & *in situ*	1597–8
" Gold-Fish	1599
" Eel	1863
" Sturgeon, Sec....	2096
" Dog-Shark	2098
Starfish (young).....	2005
" Madreporic body	2006
" Pedicellariæ ...	2007
" Spine, secs	2527–9, 2030–7
Sarcina	1495
Sarcoptes	1925–6
Scalp secs.....	2025–6
" negro	2131
Statoblasts of Cristatella.	2508
Sarcoma (Giant cell)....	573
" Spindle cell....	1496
" Cystic.....	1731
" Osteo	1792
" Round cell....	2804
" Melanotic	2987, 2820
Snails, "Palates".....	1073–8

(*Vegetable.*)	*No.*
Scales (See Hairs).	
" of Fern.....	207
"Star Polishing Powder"	2526
Starch, Corn.....	886
" Potato, and *in situ*	887–8
" Canna, pure and commercial ..	955–6
" Wheat	980
" Rice, pure and adulterated.	1125–6
" Arrowroot, *in situ*	1699
Sanguinaria, Sec.....	710
"Star Fungus".....	730
Salicine	1536, 855
Santonine	1029
Stamens (see Flowers).	
" Lobelia.....	1367
" Salvia	1368
" Tradescantia ..	1710
" Vaccinium	1839
" Deuzia	1982
" (Petaloid)	2880, 2173
" Willow (to ovaries	2740–4
Scalariform vessels.....	2930, 2885
"Santa Monica" deposit	2891

(*Mineral.*)

Satin Spar.....	589
Sand, Oolitic	987
" Auriferous	2820
" Sonorous.....	2907
Stalactite	2821, 1983
Slag from iron furnace..	2256
" " copper "	2741

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better, and in very large collections practically better for finding any specified slide that one knows he wants; but are not better, nor even as good, for assisting him to decide what he wants from among many.

In this catalogue, pages for from 1000 (the smallest number really worth starting with) to 3000 objects, with an adequate appendix of blank pages, and an alphabetical index as described above, can be readily bound in a volume of convenient size. For larger collections of any required limit, they may be bound in more than one volume. The serial list may be divided without disadvantage, say 4000 or 5000 to a volume, but the appendix and index should then be bound in a volume by themselves or separately, if too large for combination. In very large sets, the animal, vegetable, and mineral kingdoms may be indexed in separate volumes.

This system of building up a useful descriptive catalogue at a merely nominal expenditure of labor is the outgrowth of the writer's experience, during some thirty years past, in trying to make a constantly increasing collection of slides as useful as possible, as objects of reference in teaching as well as in study, and with the least possible cost of time and labor. Its details are easily varied for adaptation to special wants or changing manners. It has been of service to many of the writer's friends as well as himself; and it is offered in the hope that, directly or indirectly, it may be of service to others working in the same field.

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WARD'S SLIDE-CATALOGUE.

(Left-hand Page.)

OBJECT.

Slide No.

- **c.** Common Name.
- **s.** Scientific Name.
- **N.** General Character or History of Object. Special points shown, Illumination or powers required, Reference to Authorities, etc.

SOURCE.

- **h.** Habitat or Locality.
- **o.** Collector (Presented, Purchased, Exchanged, etc.)

50	c	h
	s	o
	N	
51	c	h
	s	c
	N	

(Right-hand Page.)

PREPARATION.

- **p.** Preserved (Hardened, Macerated, Decalcified, Injected, etc.)
- **ct.** Cut (Embedded, Frozen, Microtome), Teased, etc.
- **st.** Stained.
- **cl** Cleared.

MOUNTING.

- **m.** Mounting Medium.
- **oc.** Cell and Cement.
- **cg.** Cover-glass Thickness. **d** Date. **lc.** Location in Cabinet.
- **r.** Repairs or Disposal (Broken, Cement run in, Air in, Given to or Exchanged with, etc).

50	p	m			
	ct	oc			
	st	cg	d		lc
	cl	r			
51	p	m			
	ct	oc			
	st	cg	d		lc
	cl	r			

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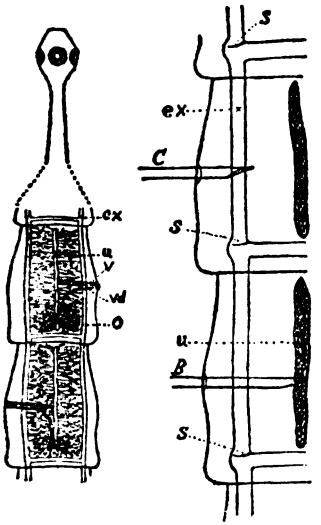
THE TAPE WORM.

Methods of Preparation for the Museum and the Microscope.

By J. M. Stedman, Cornell University, Ithaca, N. Y.

Hoping that it may be of benefit to others, I venture to give some methods by means of which I was enabled to preserve a tape-worm (*Tænia saginata*, Gœze, *T. medio-canellata*, Kuch.), and at the same time show its anatomy most beautifully.



For museum purposes, the excretory (water vascular) system (ex) of the whole worm, or the latter part of it in which the uteri are fully developed, is injected with a fine injecting mass, such as is used for the injection of cold-blooded vertebrates. This can readily be accomplished by inserting the end of a canula (C), which should be cut off obliquely, into the generative cloaca or opening of the vagina (v), in which operation the excretory canal (ex), will be cut, and if the canula be not inserted too far, the whole of the excretory system caudad of the canula will be injected without any difficulty. This operation of injecting will demonstrate the presence of valves (s) in the canals at the caudal part of each proglottid or segment of the worm. Thus it is that only that

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portion of the excretory system caudad of the insertion of the canula will be injected, the valves (s) preventing the flow cephalad. The worm, after being injected, is then washed in water and put into 75 per cent. glycerine to which has been added a few drops of acetic acid. In the course of a few hours the worm will become transparent, and in addition to the excretory system, the uteri (u), ovaries (o), vagina (v), and vas deferens (vd), which change but little, if at all, will be distinctly seen. If a worm or a portion of it thus treated be suspended in a bottle of 75 per cent. glycerine so as to be easily seen, and the bottle thoroughly corked, the specimen will keep indefinitely. A tape-worm thus prepared, is, indeed, a beautiful as well as instructive object.

For microscopicical purposes about five centimeters of the worm treated the same as for the museum was mounted in glycerine-jelly in a cell in the usual way. Another portion had the uteri (u) and ovaries (o) of several of the segments also injected. The operation of injecting the uteri and ovaries is very simple, and is readily accomplished by forcing the canula (B) further in than in the case of injecting the excretory system, so that the canula reaches nearly the center of the segment. Occasionally the uteri can be injected by forcing the canula but a little way into the vagina (v). Segments in which the uteri are fully developed and extended with ova need not be injected, as they are quite distinct, but the ovaries and the immature uteri are not seen well unless injected. A few centimeters of the worm in which the excretory system is injected with one color and the uteri with another, form very beautiful preparations.

Another preparation for the microscope was made by placing a few segments of the worm in Müller's fluid for three days, after which it was injected, both the excretory system and the uteri (for unless the uteri are injected they will not be seen in this preparation), then hardened in 50,

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75 and 95 per cent. alcohol, cleared in turpentine three parts, carbolic acid two parts, and mounted in balsam. These preparations in balsam require more work than those in glycerine, but it may pay in the future, the greater permanence compensating for the extra labor.

Mature segments of the worm were hardened in Erlick's fluid and alcohol, and cut into transverse, and longitudinal serial sections. The longitudinal sections show the valves (s) of the excretory system in their natural position.

The nervous system may also be studied by means of sections.

Ova were taken from the last segment, placed in picric acid and alcohol (50 per cent. alcohol, 100 cc: picric acid, 1.5 gramms,) for 30 minutes, and mounted in balsam.*

The only published methods of which I am acquainted for the preparation of the tape-worm, is in "*Manuel de Microscopic Clinique*," par G. Bizzozero et Ch. Firket, in which a process consisting of drying is described.

* Each statement made in this article was illustrated by a mounted specimen.

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A DESCRIPTION OF ERGASILUS CHAUTAUQUAENSIS,

A New Species of Cope oda, and a List of Other Entomostraca found at Lake Chautauqua, in August, 1886.

By Charles S. Fellows, F. R. M. S., Minneapolis, Minn.

During the last session of the Society, held at Chautauqua, in August, 1866, a gathering was made of the surface-swimming entomostraca of Lake Chautauqua. This gathering was entrusted to my care for examination. For various reasons the examination has not been as thorough as it should have been, but I am able to make a partial report, and am in hopes before another meeting to supplement it with additional matter.

Among the free-swimming Copepoda one was found that attracted my attention from its remarkable beauty of form and coloring, which, upon close inspection, proved to be a new species of parasitic copepod. Its host has not yet been ascertained. This species resembles *Ergasilus sieboldii*, Nordmann, and also *E. centrarchidarum*, Wright, but differs from both in many particulars.

Its general shape is that of a rather narrow cyclops, and the egg sacs (which in this genus are usually very long) are almost a perfect counterpart of some of our species of Cyclops, being of an elongated oval form. Its length is from .86 to .90 mm., exclusive of the furcal bristles, being nearly twice as long as *E. centrarchidarum*. The abdomen is composed of five segments, the rami of the furca being as long as the three preceding segments. As in all species of this genus, the second maxilliped is absent in the female, but in the male it is a large, prehensile organ.

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ERGASILUS CHAUTAUQUAENSIS n s.

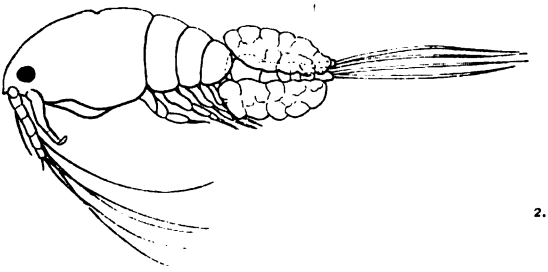
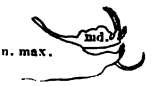
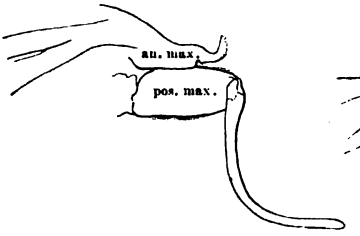


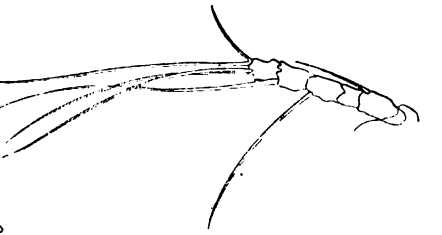
Fig. 1. Female — Side view mag. 45 dia.



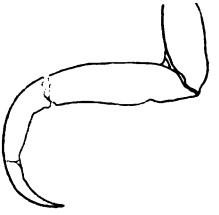
2. Mandible; maxilla and maxillepede *of female.*



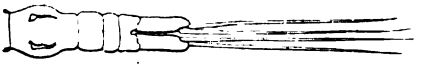
3. Anterior and posterior maxillipedes *of male.*



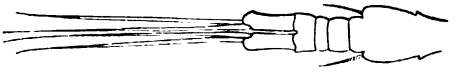
4. Anterior antenna.



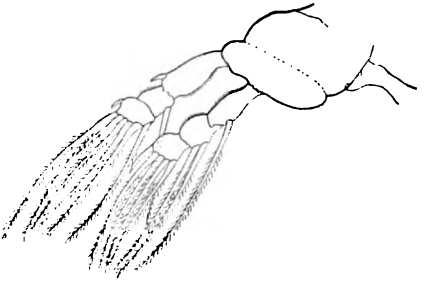
5. Posterior Antenna.



6. Abdomen, furca and furcal setæ *female.*



7. The same *male.*



8. One of the third natatory feet.

(Figures 2 to 8 inclusive magnified about 125 dia.)

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Ergasilus Chautauquaensis, n. sp.: Cephalothorax slender, three times as long as wide, composed of six segments, the first and second fused. The abdomen is composed of four segments, besides the furcal rami, which are as long as the three preceding segments. The furcal setæ are two, of nearly equal length, attached to the inner margin of the ramus; the outer of the two is the stoutest; they are nearly twice as long as the abdomen; the anterior antennæ are six-jointed and are armed with setæ; those on the fourth, fifth and sixth joints being very long and reaching to the last thoracic segment. The posterior pair are four-jointed and resemble those of *E. Sieboldii*. The basal joint is *not* inflated as in *E. centrarchidarum*. The mouth parts are difficult to make out, but appear somewhat similar to *E. Sieboldii*, as described by Claus. The head of the first maxilliped is much smaller and not so straight as in *E. Sieboldii*; the mandible appears to be without palp. In the female the posterior maxilliped is wanting, but in the male it is a large, three-jointed, prehensile organ; the basal joint is very short, and the second is quite long and stout and is cilliated on its margins. The third segment is long, slender and curved, its distal extremity being blunt and rounded. The natatory feet are five pairs; the four anterior being biramous, and, except the fourth (where the ramus externus is two-jointed), are three-jointed. They are variously armed with spines and *plumose* setæ. A particular description of their armature is as follows: The basal joint is not cilliated. The basal segment of the ramus externus of each of the four anterior natatory feet is armed with a spine on the outer distal extremity and cilliated inwardly. The middle segment is outwardly cilliated and inwardly carries a *plumose* seta. The apical segment of the first foot has upon its outer distal extremity two spines and apically five *plumose* setæ, while the second and third feet have each one spine and six setæ, and the fourth foot one spine and five setæ.

The basal segment of the ramus internus of each of the

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four anterior natatory feet is outwardly cilliated and inwardly carries one *plumose* seta.

The middle segment of the first foot is outwardly cilliated and inwardly carries one *plumose* seta, while the second and third feet are outwardly cilliated and each inwardly carries two setæ. The apical segment of the first foot has upon its outer distal extremity two spines and apically four setæ; the second and third feet have each one spine and four setæ, and the fourth foot has one spine and three setæ.

The fifth foot is either wanting or is represented by a rather long spine attached to the sternal plate of the last thoracic segment.

The blue coloring of the entire center (longitudinally) of the otherwise very transparent body makes it a conspicuous object.

Of the other Copepoda found, one (*Epischura locustris*) is comparatively new, being described by Forbes, in 1882 (Am. Naturalist, 1882, page 648), from Lake Michigan, at Traverse Bay and Chicago, also at Geneva Lake, Wis. Only one specimen of this form was found, but as I found others in Niagara river, at Buffalo, I think it may have been overlooked in the gathering. Immature forms of this genus have been found on the surface of Lake Tahoe, California. This and one or two species of Diaptomus similar to those found in Lake Michigan, but with slight varietal difference, and two of Cyclops, one of which I think is new, comprise the Copepoda.

The most conspicuous Cladocera found was the beautiful *Leptodora hyalina*, first noticed in this country by S. I. Smith (who found, however, only a mutilated specimen) in Lake Superior, and later by Prof. S. A. Forbes in Illinois and Wisconsin. I have also found in Cedar Lake, Indiana, the same species. I found no mature specimens, however, of this genus, and therefore have not been able to study it critically; but from what I have seen of the immature forms it does not

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differ from the European species as described by Liljeborg and Wiesmann.

A species of Daphnia, which closely resembles *D. Cederstromii*, was the most abundant of the Cladocera. All varieties of "hood" were represented, and it was evidently like that described by Forbes as a variable Daphnia approaching *D. hyalina* on the one hand and *D. retrocurva* on the other.

Daphnella brachyura, Levin, and *Chydorus sphaericus* O. F. Miller, were abundant, and differed but little, if any, from the European species.

I also found a species of Ceriodaphnia, which I am unable to determine, and think it is undescribed; also a single specimen of *Bosmina longirostris*.

I have no doubt that other forms have a place in the Chautauqua fauna, but the limited time spent at the lake, and the fact that no dredging from the bottom was examined, accounts for the few number of species.

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NOTE ON A NEW ROTIFIER.--GAMPHOGASTER AREOLATUS.

By C. M. Vorce, F. R. M. S.

At the meeting of this Society in Elmira in 1882, I mentioned, giving a brief description and figures, a new, and in many respects remarkable Rotifer. (Vol. V, page 187.) At that time I had had only sufficient time for the examination of the literature of the subject to satisfy myself that this Rotifer was new and undescribed, and also to learn, by search for additional specimens, that it was extremely rare. A single specimen only had been found, taken living in filterings from the water of Lake Erie, at Cleveland, Ohio, and was kept under observation in a live-box so long that it was fairly to be presumed that all the details of its structure which could be observed in that one specimen in the living state were noted and recorded.

Subsequent search and correspondence confirm the conclusion that it was never described or figured before the publication referred to, nor observed by others so far as can be ascertained. I therefore deem it proper to now formally describe and publish the genus and species, both of which are new, and although the limited search for new specimens which I have been able to make has not yielded any additional specimens, the original form having been kept and studied so long and the carapace permanently preserved, I feel fully justified in publishing the description from that specimen, especially as precedent is not wanting for such course. The description both of genus and species, but particularly the

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former, must necessarily be to some extent provisional, although the characters of the type specimen are so marked and distinct as to render the necessity for revision quite unlikely. I therefore propose the following description:

GOMPHOGASTER. gen. nov. Carapace thick, box-like, enclosing the animal completely except for a narrow slit-like opening upon the anterior ventral portion, extending in the type specimen for about one-half the total length of the carapace; carapace cuneate in both dorsal and lateral aspects, doubly so in the type specimen; carapace triangular in transverse section, the ventral side apical; foot strong, jointed, and usually retracted within the carapace, toe apparently single; trochal disc apparently single, cilia robust set in a single (?) marginal row, disc not much expanded beyond carapace when extended; a stout, retractile horn-like process protruded from each dorso-lateral corner of the carapace when trochal disc is extended; eye spot single (?) located well below trochal disc; mastox small but apparently powerful.

Animal very active, strong and apparently voracious. Habitat, fresh water.

Gomphogaster areolatus, sp. nov. Carapace thick and strong, dark colored, marked all over with areolar depression, very much resembling the markings of Heimaulus, or Trinaciea; sides slightly concave, a deep plicate furrow across back at widest part, from which two deep sub-central furrows and two shallow sub-marginal furrows extend upon dorsal surface to posterior tip of carapace, lateral margins of the dorsal front of carapace slightly produced making the corners prominent. Foot stout, two-jointed, toes apparently single, retractile horns very slightly elevate. Eye spot not observed, but if present would be concealed by the dark carapace under usual condition of extension of the animal. Mastox near mouth. Cilia of the trochal disc robust, in a single (?) marginal row. Animal very active and strong, pushing its way among masses of diatoms and flocculent matter, and when

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entangled forcing itself by vigorous kicks with its strong foot. In water of Lake Erie, Cleveland, Ohio, a single specimen, July 2, 1882.

EXPLANATION OF PLATE.

FIG. 1.—Ventral aspect.

FIG. 2.—Dorsal view, showing the ridges and furrows upon the back.

FIG. 3.—Side view showing foot, and the transverse dorsal folds.

FIG. 4.—Frontal view, showing oval cavity.

FIG. 5.—Frontal view, the body inclined toward the ventral aspect, showing the ridges and furrows of posterior dorsal surface.

FIG. 6.—Diagrammatic figure, on slightly smaller scale, showing supposed structure of the body and position of the parts in relation to the carapace. The *position* of the mastox observed; its structure not satisfactorily made out; the other internal parts judged of by movements thought to be seen, but not clearly made out. The dark color of the carapace and persistence of the Rotifer in keeping the dorsal surface uppermost prevented satisfactory observation of internal parts. The most violent kicks made with the foot do not protrude it further than shown in FIGS. 3 and 6, which indicate an attachment of the body to the carapace at or near the margin of the ventral cleft, as indicated in FIG. 6.

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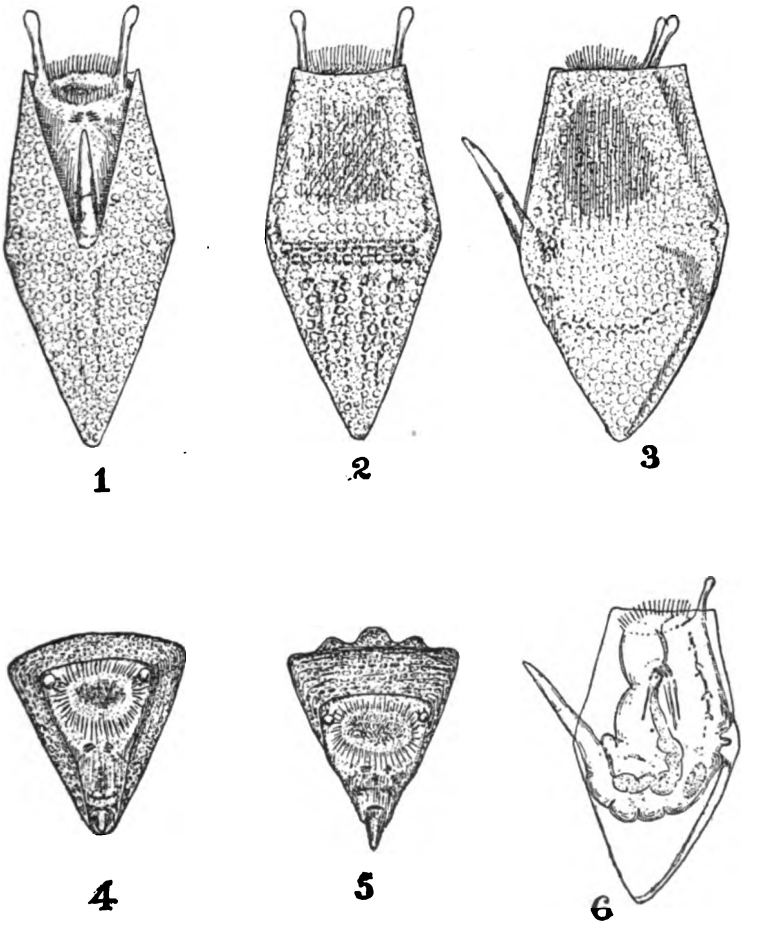

GOMPHOGASTER AREOLATUS, ×200.

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THE FALLACIES OF POPULAR BACTERIAL RESEARCH.

By George W. Lewis, Jr., A. M., M. D., Buffalo, N. Y.

I venture to say that every science has suffered more or less in its earlier development from the unprincipled and unguarded assertions of a certain well-defined class of men whose only prompter is an insatiate desire for notoriety, and whose only qualification that attracts attention to their sweeping statements is an established position in some other calling. I don't know that I should say "suffered from such assertions," rather, perhaps, should it be benefited from them; for later on it is these very statements, far-reaching and groundless as they are, that serve by their contrast to strengthen and to disseminate the underlying principles of that science. It suffers only through the delay caused by the heaping on of so much trash and the consequent sifting that is rendered necessary.

The striking features, however, of these so-called "new discoveries" and empty criticisms are (1) that they are not advanced, as a rule, until their supporters realize that the subject has assumed enough importance to warrant the attraction of popular attention to their statements; and (2) that the promulgators of the class of work to which I refer are so profoundly ignorant of their subject as the most superficial knowledge of it can well make them. I take it that Pope's expression, "A little knowledge is a dangerous thing," was meant particularly for this class of individuals. Nevertheless we all know that the more reckless and sweeping the assertion the more certain is it to attract wide-spread

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attention, and this is the only object its maker desires. It matters little to most of them how soon it is disproved, so long as timely notice is taken of it. It may be stated as a well-based induction that the more careful the investigator and the more complete his mastery over the endless practical difficulties which surround experimentation on his subject, the more certain are his experiments to give a guarded result; while extravagant and unfounded assertions are no less sure to crown the efforts of the unskilled.

These general remarks are intended as a preface to the subject in hand, for I don't believe that any science has ever suffered more for the cause of notoriety than has Bacteriology. The reason for this seems to lie largely in the fact that the science is being developed under very peculiar circumstances. In the first place it is one of the few technical sciences in which the great mass of people takes deep interest. It is easy, therefore, for those possessed of only a superficial knowledge to play upon the popular mind. If no more harm were done there would be little cause for writing. But, taking advantage of the combined deep interest, yet profound ignorance, of the masses, there has sprung up a vast number of self-styled discoverers, whose efforts, instituted solely for advertising purposes, bring little less than ridicule upon the germ theory of disease. Private laboratories, fitted up with costly apparatus, are made to stand for extensive experimental research on the part of the owners, and in their own immediate circles, at least, no opportunity is lost to impress others with the idea that they are more or less closely identified with the onward progress of the science. Another cause which undoubtedly militates against the speedy uprootal of this sort of charlatanism is the impetus given it by the outbreak of an epidemic. Here a deal of notoriety is gained at the expense of popular fear.

Scarce, indeed, are the issues of even our most representative medical journals in which more or less space is not given up to "new discoveries" in Bacteriology, always, of

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course, associating the name of Dr. So-and-So as the discoverer. Occasionally a few unimportant details relative to the peculiar circumstances under which the discovery was made, and calculated solely to enlist the popular mind, are given. Very seldom is mention made of culture or inoculation experiments, but great stress is always laid upon the microscopic appearances of the organism, and the easy or difficult manner in which it takes the stain, and perhaps other comparatively worthless data. More often, however, we are simply informed that the discovery has been made and unfortified by details that would assist subsequent observers in confirming the experiments, all else is left as the work of a prolific imagination.

Now and then the monotony of "new discovery" trash (this word in lieu of a better one) is varied by a vigorous and wordy attack upon some demonstrated and generally accepted organism. It seems unnecessary to add that the success or failure of such attacks depends largely, so far as the desired notoriety is concerned, upon the ability of the ones who make them to replace the blighted (?) remains of the old theory with a new discovery. Sometimes the devices resorted to and the claims made are, to say the least, unique. A fair sample of the indefiniteness which invariably characterizes such work is seen in the following, which appeared in the editorial columns of the New York *Medical Record*, of February 26, 1887, and upon which the editor passed a very just criticism.

The article has reference to the report of the Cholera Commission, despatched to Spain last year by the combined action of the Royal Society, the University of Cambridge and the Association for the Promotion of Scientific Research. The commission consisted of Drs. Roy, Graham, Brown and Sherrington, and a review of their report is thus given by the journal named. "Twenty-five typical cases of cholera were examined with the result that Koch's Comma Bacillus was not discovered in the intestinal canal in all the cases.

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In some this microbe was present in great abundance, in others it was far less conspicuous, while in many undoubted cases, where death occurred before the reaction stage set in, it could not be detected at all. These observations were confirmed by the results of plate cultivation in gelatine and Agar-Agar. Moreover, it was found that when present the Comma Bacilli were collected either on the surface of the mucosa or so close to it as to suggest a penetration of the epithelium after death, but in the majority the organisms could not be found in the mucus membrane or in any of the tissues or organs. These results, which are directly opposed to Koch's, are considered to be conclusive against the Bacillus having a pathogenetic relation to the disease, but it is suggested to be the cause of the premonitory diarrhœa, which is held not to be a mild attack of Asiatic cholera, but only a predisposing condition. Having satisfied themselves that the Bomma Bacillus is not the cause of cholera, these investigators similarly dismiss the claims of Emmerich's straight Bacillus to that distinction, and also state that they were unable to recognise Klein's straight Bacillus in any of their preparations."

I have also read a copy of the original report, and the above is as concise a review of it as can be given. Here we have a good illustration of the broad assertions, unsustained by reliable evidence, to which I have referred in the first part of this paper. The report is based upon an examination of only twenty-five cases. Compare this with Koch's three years of uninterrupted study in cholera-infected localities, before a single utterance was given to the world. The commission is frank to acknowledge that its report is somewhat premature, further investigation being needed, especially in the line of artificial cultivation. Its hasty dismissal of Koch's theory is unwarranted, and savors strongly of English prejudice. Its reasoning, too, shows a lack of familiarity with Koch's views. Now listen to the indefinite character of what might naturally be expected to follow. "After much

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research another fungus was discovered, which is believed to be pathogenetic. It consists of granular masses and a delicate mycelium, which could not be stained without difficulty, and was pronounced by Messrs. Vine and Gardiner to belong to the Crytridiaceæ, a class which includes many rapidly growing and virulent parasites of vegetables. The diffiulties of its detection may have led to its being overlooked by former observers, while the objection of possible after-contamination is met and refuted."

The criticism offered by the editor is as follows: "It is, we venture to say, highly improbable that the researches above described will have much weight against the long-continued and careful observations of a trained mycologist like Koch."

The societies represented by these four men are the leading organizations of Great Britain in scientific research, but I doubt if much reliance is placed by them in the report of this commission. Their proceedings are usually both definite and trustworthy, but here both characteristics are lacking. Is there a single sentence in the whole report that conveys the slightest clue to assist subsequent observers in confirming their views? Not one that I can detect. They say the new fungus consists of granular masses and a delicate mycelium, which can not be stained without difficulty. They even go so far as to classify the new fungus. Now what fungus, pray, does not consist of granular masses and a more or less delicate mycelium? These are the two known characteristics of all fungi. With regard to the difficult staining, I shall have occasion later, in speaking of the Tubercle organism, to refer to its unreliability as a primary factor in diagnosis. It is valuable only as a confirmatory measure. Moreover, it is not by any means a constant quality even in the same organism. Subject, for example, several cultures of a given bacterium to different conditions of temperature and nourishment, each one maintained at the same throughout its growth, and you will be sure to

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notice degrees of stain-taking. This is, perhaps, less noticeable in the Tubercle Bacillus, on account of its limits of temperature being more closely defined.

The position which Bacteriology occupies to-day in its relation to health and disease has been gained solely through perfected methods of studying minutely the life histories of the various organisms known to us, and yet the implicit confidence which is placed by many in microscopic appearances as a basis of diagnosis in bacterial affections, clearly indicates the utter unreliability of a large proportion of the work done in this field. For the purpose of ascertaining beyond all doubt whether a micro-organism is actually the *causa causans* of a disease, Koch has laid down the following postulates, which are strictly adhered to by all careful workers: (*a*) The micro-organism must be found in the blood, lymph, or diseased tissues of man or animal suffering from, or dead of, the disease. (*b*) The micro-organisms must be isolated from the blood, lymph, or tissues, and cultivated in suitable media outside of the animal body. These pure cultivations must be carried on through successive generations of the organism. (*c*) A pure cultivation thus obtained must, when introduced into the body of a healthy animal, produce the disease in question. (*d*) Lastly, in the inoculated animal the same micro-organism must again be found. These steps naturally suggest a sequence in the various processes which must be adopted in a practical study of micro-organisms associated with disease.

Notwithstanding the axiomatic character of these postulates, and, in reality, the short time required for the execution of the necessary steps in each, we are, every now and then, led to believe from articles in our journals that more speedy, and quite as trustworthy, diagnosis can be made from microscopic examinations of the discharges peculiar to the diseases in question. This is particularly the case with Tuberculosis. The methods of treating the sputum previous to the examination are both varied and numerous, each

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having its devotees; but they all unite in a dependence upon difficult stain-taking as the one strong point in the diagnosis. Biedert, whose plan, as given in the *Medical Record* of March 19th, is, perhaps, the latest, claims that the smallest possible number of Bacilli, if present, can be detected. The essential features of his method are as follows: "A tablespoonful of the suspected sputa is added to twice that quantity of water and fifteen drops of a strong solution of soda. This mixture is boiled until it becomes quite fluid, and then diluted with about two ounces more of water. After being again boiled the mixture is nearly homogeneous and free from lumps and particles. If, on cooling, a thin fluid does not result more water may be added. A conical vessel now receives the mixture, and after two or three days the supernatant fluid, having deposited its bacilli, can be decanted. The sediment, after energetic stirring, can now be examined in the ordinary manner." This method is open to additional objection on account of the dangers of after-contamination.

From the comparatively healthy mouth there have been isolated various micro-organisms that, so far as known, have no pathological significance. Any uncleanly condition arising from laxity in the use of the brush or toothpick, whereby animal and vegetable matters are exposed to the process of decomposition, greatly increases the number and character or these organized bodies. The saliva, too, teems with living creatures, which, like the above under the microscope, bear a close resemblance to each other. How then, I ask, can a trustworthy diagnosis be made from microscopic examinations of sputum which contains, besides the possible Tubercle Bacilli, numerous other bacteria of like form and appearance? It will, no doubt, be claimed that a sufficient distinction can be arrived at through the staining peculiarities of the Tubercle organism. Here, however, I would say that at least two of the non-pathogenic forms from the human mouth (Weinhawer's Bacillus and Babe's Bacillus), with which the writer is acquainted, can only be stained success-

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fully by adopting some one of the methods recommended for the Tubercle Bacillus. In fact, several of the species found in the mouth were not discovered till late, on account of the difficulties experienced in staining them. Reference has been made, moreover, to the inconstancy of this quality of stain-taking, owing to the influence of different conditions of temperature and nourishment upon their organization If the Tubercle Bacillus possessed distinctive microscopic characteristics like the Anthrax Bacillus, for example, there would be cause for placing some reliance in its appearance, but, unfortunately, it has no characteristic under the microscope that is not showed equally by other bacteria in the field of vision.

If the discovery of the Tubercle Bacillus is to have any real value from a therapeutic standpoint it is essential that it be detected in the incipient stage of the disease. To do this requires not only a very exact primary method, but all the available checks that can possibly be interposed in the various steps necessary for its confirmation. The one respect in which all micro-organisms differ is their manner of growth. Here, then, is manifestly the starting point from which every reliable diagnosis of bacterial disease must proceed. Other measures of whatsoever sort are to be regarded as subservient to this, and to be of value only so far as they confirm the steps already taken.

I wish to speak, also, of the importance of maintaining an even temperature throughout the growth of the organism. It is a feature in bacterial study too often neglected, and one which, if allowed to pass unheeded through successive generations, is certain to result in more or less pronounced modifications as regards structure and development. It is a question, too, whether intentional neglect in this respect will not in time check, or even destroy permanently, the pathogenetic tendencies of many of the bacteria. Every organized body has its normal limits of temperature, be-

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yond which it cannot pass without suffering. It is a factor in their well-being next in importance to suitable nourishment. There is little doubt that these unicellular bodies are quite as sensitive to changes in temperature as are the most complicated and highly developed of the vegetable kingdom.

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ON THE USE OF THE AMPLIFIER

WITH OBSERVATIONS ON THE THEORY AND PRACTICE OF PHOTO-MICROGRAPHY, SUGGESTED BY THE DESIGN OF A NEW PHOTO-MICRO-CAMERA.*

By Geo. W. Rafter, M. Am. Soc. C. E.

In the proceedings of the American Society of Microscopists for 1884, there occurs a paper on Photography with High Powers by Lamplight, by Hon. J. D. Cox, of Cincinnati, Ohio. On page 100 I find the following: "Without pretending to be sure that my method is the best, I will still say that I have thus far got the best results by using the No. 1 ocular in the microscope and no other amplifier. It seems to me that after correcting the objective with care so as to present the best results to the eye directly, the satisfactoriness of the image which is thus produced, is best kept by using both objective and eye-piece in photographing, precisely as in looking, and with the same length of tube; changing nothing but the fine adjustment, to correct the focus for the position of the camera-screen. Such, at least, is the conclusion I have tentatively reached."

The distinguished standing of the author of the above, among microscopists, both American and foreign, gives it an authority which would not obtain if coming from a microscopist of less universal fame. It is with some little diffidence, therefore, that I presume to criticise his statement, and I cannot but regret that Mr. Cox has not given more in detail the experiments which have led him to this conclusion.

* Originally published in the Rochester, (N. Y.) *Odontographic Journal* for July, 1887, also reprint 1887. Abstract with additional matter by the Author.

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Certainly his conclusions are exactly the opposite of that indicated by theory, and they are equally opposed to the practical results obtained by Dr. Woodward.

In recent times the tendency to simplify photo-micrography has led to an ignoring of some of the fundamental principles of the art, and this present paper will have amply fulfilled its mission, if it succeeds in directing the attention of the more advanced workers to certain indispensable conditions, without an observance of which success cannot possibly be obtained.

Dr. Woodward, as long ago as 1866, pointed out the proper method of procedure for obtaining the best photo-micrographic results, and at that time laid down general principles which represent so far as the English literature of the subject is concerned, the first *complete* system of photo-micrographic practice.

I am aware that previous to this time much creditable work in photo-micrography had been done in England and on the continent of Europe, but Dr. Woodward's system is far more complete than anything bearing upon the subject which had been produced up to that time. It is specially valuable that it is the first recognition of the value of the achromatic concave amplifier for obtaining additional amplification instead of by use of an eye-piece, and it is due to this discovery* alone that he was able to make in the later years of his work the magnificent series of photographs of the test diatoms, and other difficult subjects which still remain on the whole unequalled by the productions of any other worker, and are likely so to remain for a long time to come, monuments to the photographic genius of Dr. Woodward. To him, therefore, may fairly be awarded the credit of having made photo-micrography as a practical art possible, of having, indeed, put it on such a basis, as to render easy the production of photographic representations of micro-

*Dr. Woodward states that he obtained the suggestion of such use of the amplifier from Mr. Lewis M. Rutherford.

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scopic objects on a large scale. Previous to his day it was in effect only a scientific plaything.

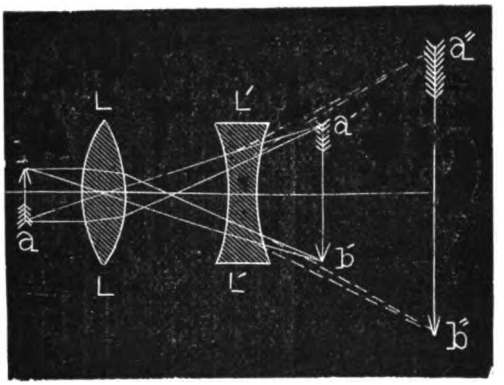

Fig. 1.

Referring to Fig. 1, let L L be a double convex lens representing the lens system of an objective ; a b an object between the lens and the principal focus on that side; a a′ and b b′ secondary axes drawn from a and b respectively through the optical center. Also consider parallel rays of light radiating from a and b; these, after passing through the lens are refracted to a″ and b″, meeting the secondary axes a a′ and b b′ at those points; conjugate foci are formed at a′ and b′ whose corresponding initial foci are a and b. From this results the formation at a′b′ of a real magnified image of the object a b. It is evident from the figure that the size of a′b′ is to the size of a b, as their distances from the optical center respectively.

Suppose, however, that the double concave lens L′L′ is so placed as to intercept the rays from a b after they have passed through L L and before coming to a focus at a′ and b′. By such interception the rays are made more divergent, and the image instead of being formed at a′b′ is formed at a″ b″. It is moreover apparent from the figure that if L′L′ is placed in any other position in reference to L L, that the image a″b″ will still be formed, and that it will advance or recede from L L as L′L′ advances or recedes.

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It is also apparent from what has gone before that if the object a b is brought nearer to L L, the divergence of the axes a a′ and b b′ will be increased and a new image may be farther projected to any required distance (within limits) by proper placing of the lens L′L′.

Fig. 1 therefore illustrates the formation of photographic images when photographing with the naked objective or with the concave amplifier.

The practical fact has been repeatedly observed that when using the naked objective in photo-micrography, for considerable extension of the camera, the objective is brought so much nearer the object than its normal working distance that aberrations are introduced which frequently completely destroy the sharpness of the image, or in the case of high powers, the objective is brought so near the object, that, with the short working distance which such objectives always have, it is found impossible to focus at all for any considerable extension of screen. This difficulty has usually been met by the use of the eye-piece, but this, unfortunately, introduces an entirely new set of conditions and difficulties, which have been found more impossible to reconcile than the comparatively simple ones which occur when using the naked objective.

From the diversity of opinion which prevails as to the relative advantages of these different methods of projecting the photographic image, I have been led to suppose that the theory of the matter is not generally understood, and the optical part of this paper is therefore an attempt to set forth the theory in its proper light.

With the amplifier, however, the difficulties above enumerated are immediately removed, all that is required being to find by trial such a position of L′ L′ as will give, for any position of a b with reference to L L, a distinct image at any required position of the screen a″ b″.

Fig. 2 illustrates the principles involved in the projection of an image with the eye-piece. a b is the object, and L L

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the lens system of the objective as before, and a′b′ the real aerial image formed by the objective. L′L′ represents the eye-piece. The rays after coming to a focus at a′b′, and forming the real image a′b′, diverge, and, on undergoing refraction in passing through L′L′, are again brought to a focus and produce the image a″b″, which may be received upon a screen. From the preceding principles it is evident, that in producing a photographic image by this method, there is not only great loss of light, but also great loss of distinctness of the image. From the theory of the formation of conjugate foci on principal and secondary axes it is apparent, that, on and in the vicinity of the principal axis, the image is more distinct than at points off of the principal axis, and that the farther any point of the image is removed

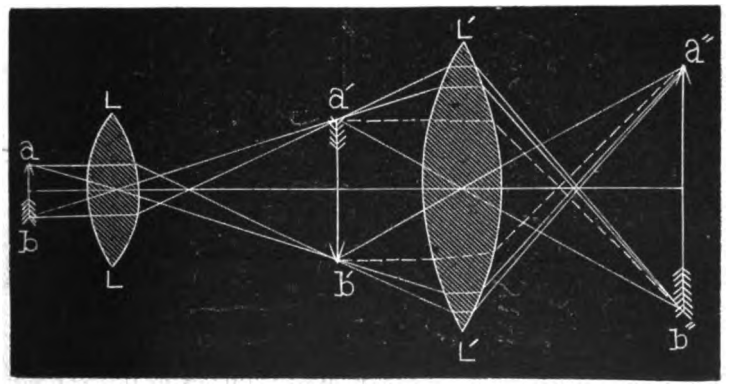

FIG. 2.

from the principal axis the greater will be the indistinctness. Again the indistinctness of the image will increase in proportion to the amount of divergence of the secondary axes passing through the extremities. Moreover the Huyghenian eye-pieces, which are generally used for this purpose, are not really achromatic combinations. Their apparent achromatism is produced, not by bringing the red and violet images into true coincidence, but by causing one to cover the other as seen from the position of the observer's eye.

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But the conditions, which obtain in vision through an eye-piece are quite different from those which obtain when an image is projected through an eye-piece upon a screen. So different are they, indeed, that when the microscope is focussed for ordinary microscopic vision through an eye-piece, there is no position of the screen, which will give a distinct image without changing the position of the focus, either of the objective or the eye-piece, and such change of necessity introduces conditions which effectually destroy the apparent achromatism of the eye-piece as used for microscopic vision. Fig. 3 in comparison with Fig. 2 will show the difference between projection upon a screen and ordinary microscopic vision through an eye-piece. Referring to Fig. 3, let L L be the eye lens of an eye-piece, E E is taken to represent the lenses of the human eye and R R the retina. r r′ r′′ r′′′ are rays of light which have undergone refraction through the field lens of the eye-piece, and which are brought to a focus at a and b, forming the real image a b. After forming the image a b the rays r r′ r′′ and r′′′ pass on towards L L and are farther refracted, after which they pass into the eye E E, forming on the retina R

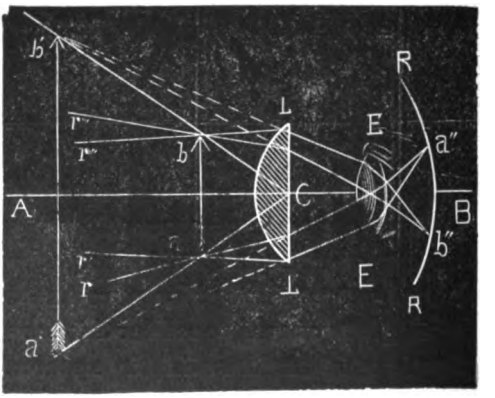

Fig. 3.

R the image a′′ b′′, which by the theory of vision is seen

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under such a visual angle as to apparently project it to a′b′. The conditions for distinct vision with the eye-piece are that the rays r r′ r′′ and r′′′ after refraction by the lens L L shall become *nearly* parallel, but converging towards A enough to meet the secondary axes through a and b (that is, the axes a C and b C) at some points, a and b′, where the virtual image a′ b′ is formed. In microscopic vision through an eye-piece, therefore, we do not see the real imageof the object, but an image of an image, or the virtual image.

It is apparent moreover from an inspection of Fig 3 that under no condition could the rays r r′ r′′ r′′′ be made to converge after passing through L L; they are made divergent, and, will always remain so, without another lens is interposed, or else that the relative positions of eye-piece and objective are so readjusted as to cause them to converge after passing through L L. It follows from this that when the adjustment is made for vision there is no position of the screen which will give a sharp image. The truth of this may be readily demonstrated by experiment, when it will be found that with the screen very close to the eye-piece an indistinct image is formed, and, leaving the relation of eye-piece and objective the same, the farther the screen is removed from the eye-piece the more indistinct the image becomes. This accords perfectly with the theory as exhibited by Fig 3.

Referring again to Fig. 2, it may be seen that the converse of the above proposition is equally true that, when the adjustment is made for the projection of an image, it is equally impossible to get microscopic vision.

We also see by comparing Figs. 2 and 3 why the change of combination necessary to project an image upon the screen must, of necessity, destroy the apparent achromatism of the eye-piece. The correction is made, as above stated, for bringing the violet and blue spectra, not coincident, but one above the other, when the eye-piece is used for

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vision. But such use renders rays divergent, which in projection of an image are required to be convergent. It would seem to follow, therefore, that these spectra will separate in proportion, interfering materially with the perfection of the image. The essential truth of this statement may be readily determined by experiment.

In any case, in photographing with the eye-piece, changes in distance of screen from object should be compensated for, not by changing the focus of the objective, but by changing the position of the eye-piece. The objective being once set to such position as gives the best image with normal length of tube and eye-piece, should then be left undisturbed, and the adjustment for position of screen made by change of position of eye-piece.

This practical difficulty in the use of eye-pieces for projection appears to have occurred to the eminent German optician, Carl Zeiss, of Jena, as I find in his recent catalogue an account of a series of what he calls "projection eye-pieces." It would have been some little gratification to optical students had Herr Zeiss seen fit to set forth more clearly the optical principles involved in these new projection eye-pieces. Possibly he has done so in some of the German periodicals, but thus far I think no such exposition has appeared in any of the American or English microscopical journals.

Lacking such complete exposition of the optical principles involved, a few provisional criticisms are offered.

In the first place, calling them eye-pieces is evidently a misnomer, as, referring to the above description, it appears that they cannot be used for microscopic vision. The preliminary adjustment of the specimen must be made with an ordinary eye-piece, after which the projection eye-piece is placed in position. This changing of eye-pieces is in itself a serious objection in high power work where the slightest disturbance of the apparatus leads to such disarrangements as must necessitate a new adjustment. All practical photo-mi-

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crographers will agree with me, therefore, that any process which necessitates the removal of one piece of apparatus, and the substitution of another in its place is for high power work fundamentally defective. The inevitable disturbance of apparatus which will occur in making such changes, leads not only to loss of time, but usually to deterioration of the negative. Nor do I in making this criticism overlook the fact that the focusing of the object is, according to Herr Zeiss' description, completed after such change of eye-pieces. The criticism is made on the broad ground that any substitution of apparatus whatever, is undesirable, provided the best results can be obtained without such substitution. That such results can be obtained in a more satisfactory way, I will attempt to show in the sequel.

Again, the description in general terms answers to that of a Huyghenian eye-piece, that is, they consist of a convex lens (the field lens presumably), and a compound system (the eye lens presumably), with a diaphragm between them. They differ from the Huyghenian eye-pieces in this, that the compound system is truly achromatic, and that it is made adjustable in reference to the diaphragm. Such adjustment evidently having reference to the bringing of the rays to a focus as shown in Fig. 2.

I have no doubt but that the making of such an achromatic combination would materially improve the performance of eye-pieces when used in photo-micrography. However, the objection to the use of *any* eye-piece still remains valid. The crossing of the rays after passing through the eye lens, and the consequent great obliquity which they attain, must still result in unnecessary loss of light and considerable impairment of the sharpness of the image. Certainly until some expert worker shall have actually produced by such an arrangement photo-micrographs finer than those made by Dr. Woodward, we are justified in assuming that

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the use of the simpler optical combination of the adjustable achromatic amplifier is, on the whole, preferable.*

It farther appears, from this point of view, that Herr Zeiss has unnecessarily complicated his projection eye-pieces by making the compound system adjustable with reference to the diaphragm. The really important point, it would seem, was to make them achromatic and then, having once set the objective to normal working distance, to compensate for changes in position of screen by withdrawing the eye-piece. From present information it would appear that equally satisfactory results could be so obtained with some little gain in the matter of simplicity.

At any rate the result of a trial of this appliance will be awaited with no small degree of interest.†

In order to exhibit with clearness the relation between working distance and length of tube I have taken a Bausch & Lombe three-inch professional objective of 12° angular aperture, and made the approximate measurements recorded in the following table. They serve to exibit, in a general

* In the J. of the Roy. Micr. Soc., for Feb. 1887, page 20-34, Prof. Abbe has a paper on Improvements of the Microscope with the aid of New Kinds of Optical Glass. Referring to the use of projection eye-pieces, rather than the amplifier, for correcting the aberrations produced by distance of the screen, he cites as a chief objection to such use of the amplifier, the "minute and troublesome manipulation in the adjustment, if a good correction of the objective is to be secured."

Attention may be directed to the fact that the addition to the photo-micro-camera of specific mechanical appliances for instantly making this correction, entirely disposes of the above objection, and to the farther fact, that, inasmuch as Prof. Abbe has not given any other objections to the use of the amplifier for this purpose, it must be considered that the above is the chief objection that can be urged.

† Am. J. Sci. and Arts, 1861, xxxii, 185. On the practical Application of Photography to the Microscope, by Prof. O. N. Rood.

Prof. Rood states that in the course of his investigations he had three eye-pieces constructed. No. 1. on the general Huyghenian plan, the eye lens being an under corrected *achromatic*, in which the distance between the eye lens and field lens could be varied at pleasure, as advised by Amici. Prof. Rood says the general performance of this eye-piece seemed somewhat better than the plain Huyghenian eye-pieces with which it was compared.

No. 2 was a Kellner eye-piece in which the distance between the lenses could be varied.

The performance was good.

No. 3 consisted of two achromatic lenses combined in the Huyghenian manner. This, I infer from Prof. Rood's statement, to have been inferior.

It appears, therefore, that Herr Zeiss' projection eye-pieces hardly have the merit of novelty.

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way, the great influence on working distance exercised by length of tube. These measurements are made with the naked objective:

Magnification = Diameters.	Length of Tube = Length of Camera in Feet.	Working Distance in Inches.
———	0.83*	1.96
———	1.15	1.67
10	1.97	1.44
20	3.81	1.33
30	5.64	1.29
40	7.47	1.27

It appears from this table that in extending the camera from ten inches to seven and one-half feet, the working distance of such an objective is reduced from 1.96 inches to 1.27 inches, or 0.69 of an inch.

The normal working distance of a wide angled four system homogeneous immersion $\frac{1}{8}$ objective is about 0.02 of an inch, and of a similar $\frac{1}{12}$ about 0.0133 of an inch†. If the decrease in working distance of either objective is at all proportional to focal length, it is evident that difficulty would be found in working either of them at an extension of from seven to eight feet. That such difficulty exists is within the experience of all workers in this department, but I am not aware that any precise measurements have been made of such decrease for higher powers. Approximate results can, of course, be obtained by computation.

The fallacy has prevailed to some extent that the concave amplifier may be legitimately used to increase the working distance of objectives. A little consideration will convince one, however, that such use can only result in an impairment of the image.

Again, referring to Fig. 1, consider a′ b′ as a fixed position at which it is desired to receive the image, as, for

*This measurement is made with normal working tube of ten inches, as used by the Bausch and Lomb Optical Co., who measure the ten inches from the focal plane of the objective to the diaphragm in the ocular.

†For tabulating working distances of four system homogeneous immersion objectives from $\frac{1}{4}$ to $\frac{1}{25}$, as made by Mr. Gundlach, see Catalogue of Gundlach Optical Co.

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instance, in microscopic vision a′ b′ may be taken as at the diaphragm of the ocular. With the amplifier L′ L′ in place, the image a′′ b′′ may be made to move towards the position a′ b′ either by moving the lens L′ L′ towards L L (within limits), or by moving the object a b away from L L. Such moving of a b away from L L is, in effect, an increase of the working distance of L L. It is evident, however, that by so moving a b away from L L we immediately disturb the normal working of the objective, and, independent of the theoretical condition, a practical trial will readily convince one that such increase is followed by impairment of the sharpness of the image.

By way of farther illustrating this part of the subject I have made a series of approximate measurements of the working distance of a so-called two-inch objective of a tabulated aperture of 15°. I say so-called two-inch, because, while this is the maker's designation, it appears on the application of a Cross formula* to have in fact only 1.455 inches focal length instead of two inches as called for.

The approximate measurement of the so-called two-inch is given in the following table:

l, in inches.	m	$f=\frac{m\,l}{(m+1)^2}$
48	31	1.453
60	39	1.462
72	47.6	1.451
84	56	1.447
96	63.6	1.463

Mean = 1.455.

The result f will be in the same unit as l.

The working distance of the above objective with ten-inch tube is 0.96 inch.

* See paper on the Focal Length of Microscope Objectives, by Charles R. Cross, in the Journal of the Franklin Institute for June, 1870, 401; also reprint in Month. Micr. Jour., 1870, IV. 149. Also, for farther discussion of the same subject, paper by Dr. Woodward, Remarks on the Nomenclature of Achromatic Objective for the Compound Microscope, in Am. J. Sci. and Arts, 1872. 3s, III, 406; also reprint in Month. Micr. J., 1872, VIII, 66; also Am. J. Micr., 1881, VI, 77.

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At an extension of camera screen of 6.0 feet the working distance of the naked objective is 0.76 inch.

With the camera screen still remaining at an extension of 6.0 feet, and with an amplifier of 6 inches negative focus so placed that the front of the amplifier lens is 4.75 inches back of the posterior lens of the objective, the working distance is again brought to 0.96 inch.

It follows, therefore, that this position gives the proper relation of the amplifier and this particular objective for producing the best photographic results at an extension of 6.0 feet.

The following series show the effect of amplifier in increasing the working distance beyond the normal amount.

With amplifier immediately back of posterior lens of objective (about one inch away), and with camera screen extended to 8.0 feet, the working distance is found to be 1.15 inch.

With same conditions, except that screen was at 3.0 feet, the working distance is 1.22 inch.

From a consideration of Figs. 1 and 2 and the above measurements, it appears evident that when the amplifier is nearer the objective than the position for normal working, the working distance of the objective is increased.

The converse is equally true, that when the amplifier is placed at a greater distance from the objective than the position of normal working, the working distance of the objective is decreased, but not in so great a degree as when the naked objective is alone used. For instance, with the amplifier at 12.0 inches from the posterior lens of the objective it appears that the working distance is 0.89 inch. At the same distance this objective gave, when used without the amplifier a working distance of 0.76 inch as above stated. The importance of having ready means of adjusting the amplifier is therefore rendered apparent by these measurements.

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In order to get such ready means of adjusting the amplifier and to develop a photo-micro-camera which would answer all the demands which might be made upon it, I have designed the apparatus shown in Fig. 5. This is really a photo-micro-camera complete within itself, and not a microscope and camera combined. I found, early in my experience as a photo-micrographer that one instrument could not be made to do the work of two, and that it was only possible to use photography as a real aid to microscopical investigations by having photo-micrographic apparatus, which, in addition to being always ready, also possessed the quality of easy adjustment to any and all kinds of work. The present design possesses not only all these qualities, but it can also be furnished at a price quite within the reach of any person really desiring such an aid to scientific investigation.

A reference to Fig. 4 in conjunction with Fig. 5 will show the novel points.

A in Fig. 4 is the stage, B is a nose-piece which carries the objective and also a removable collar carrying the tube C, which is supported by a removable pillar shown in Fig. 5. Inside the tube C is a second tube made to work back and forth very easily and carrying at its lower end a right angle prism set for total reflection. This tube is of such a length as to give, when in position for receiving the image, from the objective through the prism a length of 10 inches measured along the optical axis. The eye-piece in the outer end has cross-hairs set in the diaphragm, so adjusted, in relation to the prism at the other end, as to correspond with cross-lines on the ground-glass of the camera screen. The tube C, therefore, gives the opportunity to examine the object under the conditions of microscopic vision, and with the cross-hairs in the eye-piece farther enables the operator to exactly center the object on the screen.

F is an adjustable tube carrying within it a second tube, which may be slid back and forth. This interior sliding tube has an adapter at the front end into which an amplifier

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may be screwed, and the whole racked back and forth by the pinion E, which also carries between the thumb-screw and the body a pulley over which a band can be placed for working the amplifier from the rear of the camera screen.

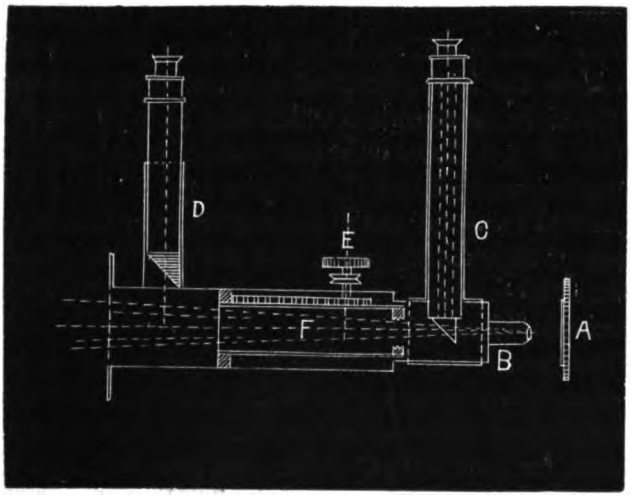

FIG. 4.

This inner tube also has a graduation on the side in order to facilitate recording the proper position of the amplifier for various extensions of the screen.

In working with high powers where it is desirable to use the amplifier, the objective is set to normal working distance by observing the object through the tube C. The operator then, from the rear of the camera, by use of the band over the pulley at E, racks the amplifier to such a position as to give a sharp and distinct image on the ground-glass, the objective in the meantime remaining undisturbed. It is of course understood that after having adjusted the objective to normal working distance, the inner tube at C, carrying the prism and eye-piece, has been sufficiently withdrawn to allow the rays of light to pass unobstructed to the camera

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screen. This gives us, almost instantly, the conditions which have been shown above to be necessary for production of the highest results, and with this apparatus the most difficult tests are easily photographed.

When working with low powers, the amplifier is not essential for the production of sharp images, and the tube C and nose-piece B are removed by simply slipping off the collar from the nose-piece and unscrewing the nose-piece from the body, an operation which may be performed in a moment. Fig, 6 shows these parts when detached.

After removing B and C, the inner tube F is drawn forward so that the front end of it occupies, approximately, the position of B when in place, and the objective is screwed into the adapter in end of said tube F, which in high power work carries the amplifier.

D is a second tube back of F, with prism and eye-piece with cross-wires, precisely as in C. With this tube the object is examined and centered on the ground-glass, as above described, for work with C. After such centering, the focusing is completed either by use of the band passing over the pulley at E, or by the use of the long rod and fine adjustment to be described below. The inner tube at D is shown as dawn back in such position as to allow the rays of the light to pass unobstructed to the screen.

The camera itself has both bellows and base made in sections each two feet in length. The sections of the bellows can be readily removed, or additional sections inserted, when great extension is required.

A section of the base is shown in the foreground of Fig. 5 and the method of attachment may readily be inferred from the cut. The brass rod for working the fine adjustment, is shown passing through the middle of said section of base in foreground of Fig. 5. The ends of this focusing rod are attached by an automatic coupling so made that there is no chance for lost motion.

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The plate and screen holder is racked back and forth as clearly shown in cut.

On the side of the base, is a graduation in feet, tenths and hundredths of a foot which enables one to record positions of the screen for producing given magnifications easily.

The fine motion is communicated to the stage, and not to the objective, as is clearly shown in Fig. 5.

The camera, as shown in the cut, admits of an extension of 8 feet, and sections of base and bellows similar to those above described can be added, extending it almost indefinitely. The extension above given will, however, answer all ordinary demands.

In its present form the camera takes a 6½ × 8½ plate, and all sizes less than that down to the smallest.

This apparatus has been specially designed with reference to doing photo-micrographic work of a high character with the greatest possible economy of time. It is for this purpose that the second prism tube has been added specially for low power work without the amplifier, and I have no difficulty in making with this camera a half dozen negatives in an evening, when working with lamplight and the amplifier, or from eight to ten in the same time when working with low powers and without the amplifier, in each case doing my own developing. In working by sunlight, where much shorter exposures are required, the same length of time gives an additional amount of work.

In case one has an extra microscope, the new apparatus for working the amplifier may be adapted to it at moderate expense, and by construction of the bellows and extension arrangements, as above described, the more important advantages of the camera gained.

For rapid work the camera should be placed on a shelf on one side of the room, at such a height as to bring the horizontal prism tubes level with the operator's eye. The position of the camera at one side of the room insures economy of space and does away with the objection that the

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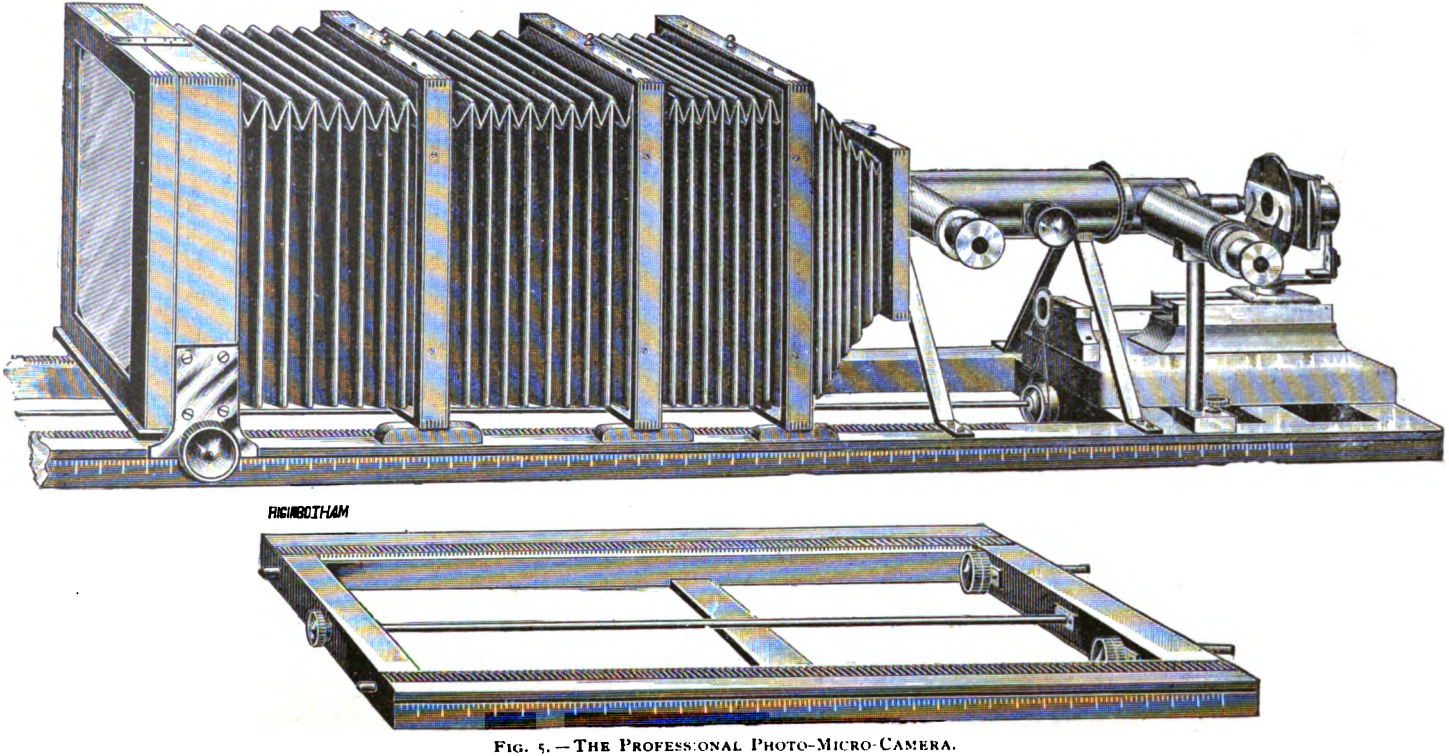

FIG. 5. — THE PROFESSIONAL PHOTO-MICRO-CAMERA.

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camera, even though of considerable size, takes up much room.

When it is intended to work by lamplight only, it will not matter which side of the room is used for the purpose, and the operator may locate the camera to suit his surroundings; care must be taken, however, to have the graduation of the base on the side away from the wall. If one has plenty of room, the best arrangement would, of course, be to erect a shelf on horses in the middle of the room, so that the camera is accessible on both sides.

When it is intended to work by sunlight, the camera must of necessity be at either the east or west side of a room with an exposure to the south, or when economy of space is of no importance it can be conveniently placed in front of a window facing the south in which may be fitted up the necessary arrangements for heliostat, mirrors or condensing lens.

In any case the surroundings will decide to some extent just what arrangement will be adopted.

The following are, as far as I know, the new features embodied in this camera:

1.—The application of specific appliances for moving the amplifier back and forth, in order to find by trial, for any given extension of the camera, the best position of the amplifier for projecting the image upon the screen.

2.—The application of two horizontal prism tubes, one for use with high powers, and the amplifier, and the other for use with low powers without the amplifier.

3.—The detachable nose-piece and prism tube for high powers only, Fig. 6.

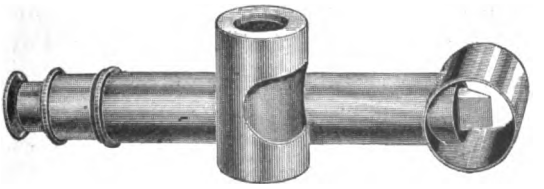

FIG. 6.

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4.—The cross wires in the diaphragm of the eye-piece of the prism tube, giving an immediate centering of the object on the camera screen.

5.—The making of the bellows in sections, in such manner as to admit of their easy removal or of a ready indefinite extension.

6.—The making of the base in sections in combination with the focusing rod, connected by an automatic coupling.

7.—The plate holder which admits of all sizes of plates from the maximum of 6½×8½ to the smallest without the use of kits.

Another new feature, which, however, is not specifically claimed, is this: If one has a prejudice in favor of photographing with an eye-piece, or if from motives of economy, one desires to dispense with the amplifier and work with the eye-piece, this may be done by simply inserting an eye-piece in the back end of the amplifier tube. For so working, an adjustable nose-piece for carrying the objective without the high power prism tube may be furnished, thus dispensing with one of the prism tubes, which, however, can be added at any time by change of nose-pieces. The object can be still centered by the posterior prism tube which is permanently fixed to the body, and the projection of a sharp image upon the screen completed by moving the eye-piece with the pinion E, Fig. 4.

The general claim is made, therefore, that this camera embodies more nearly all the conditions necessary for rapid and successful work than anything heretofore produced.

I have no doubt, however, but that a very considerable improvement can still be made, and confidently expect in view of the great interest now centering in photo-micrographic work, that the next few years will develop such improvements.

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CARDIAC MUSCLE CELLS IN MAN AND CERTAIN OTHER MAMMALS.

By Bordman L. Oviatt, B. S., Cornell University, Ithaca, N. Y.

Historical Sketch.

"Leuwenhoek and after him Kölliker," says Rauvier, "demonstrated first that the muscular substance of the heart (or myocardium) is composed of primary fibers analagous to those of ordinary striated muscle, pale and red, but differing in this that they form a multitude of branchings of a Y shape, and anastomose among themselves in such a way as to form a vast contractile network" (4,296).* Kölliker recognized the cardiac cell as the histological element of the heart, but he also held that the cells were merely fused with one another, and not cemented together (3,579). He used, as an argument for this, the fact that in the same segment, isolated by the action of potassium hydrate (M. 1),* one often finds two nuclei.

Ranvier in his (4.296) *Lesons D'Anatomie General*, in opposing this opinion, says: "You understand, gentlemen, that this argument is of no value. We know that many elimentary cells are multinucleated. But the existence, besides, of lines of cement in the structure of the branching cardiac fibers, which may be demonstrated by careful work, renders the opinion of Köllicker entirely untenable." Ranvier adds (5.541), as an explanation for the presence of the two nuclei, the following: "It is more probable that the existence of

*The first numeral refers to the number of the author's work in the bibliograp hy, the last to the page.

*M refers to the methods of work, the numeral to the number of the method referred to.

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two nuclei in the same cell is in proportion to the development. We have seen that in the cells of Purkinge the existence of two nuclei is the rule, and yet it is scarcely probable that this arrangement is connected with the complete fusion of two cells at first distinct."

In 1861 Weismann presented a paper (6.41-61) in which he says that cardiac muscle is distinguished from ordinary striated muscle in that its sarcolemma, if it has any, is so delicate that it has not yet been demonstrated; that the fibers are more slender; that they have a granular appearance; and that they have a multitude of anastomoses. He also mentions the presence of two nuclei in some cases, states that in cross section the fibers are irregular in size and shape, and calls attention to what he styles leaf-like ribs on the surface of the cardiac cells in frog and rabbit (6.44,55).

In his investigations on the human cardiac tissue (6.55) he noted tran-striæ in the cells of an embryo of four months, and stated that the striæ first appear in the substance at the periphery of the cell, after which the zone of substance next within begins to assume a similiar appearance, and so on until the entire substance of the cell, except the nucleus has become striated; also that in the embryo the fibers were made up of cells set in like tile work. They were without a membrane and were held together by tissue cement. These cells, however, which in the embryo were independent and simply placed together, became blended into a multitude of fibers (6.56). In another example, that of a child born breathing, at six months the longi-striæ were also visible. The size of the cells in this case was greater than in the one last mentioned. In both clear nuclei were visible.

In regard to the adult human cardiac muscle he says: "Fragments obtained by the use of potassium hydrate, often show the original composition of cells, for crooked lines pass over them, and, corresponding to these, the ever present nuclei are distributed." In another place he says: "The

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originally separate cells are recognizable in the adult only by slight indications."

In his *Traité Technique D'Histologie* (5.540) Ranvier says: "In subjecting to the action of potassium hydrate fragments of the heart of different mammals, cells of much larger size than those in the frog, and of different form, may be isolated. They have a smooth cylindrical surface which corresponds to that of a muscular fiber. The two bases of the cylinders are sinuous and are joined to one another by a layer of cell cement, so that they can be recognized upon those parts of the preparation where the cells have not been completely isolated."

With reference to the development he says (4.296): "The initial embryonic cell is separated into two parts, the one formed of nuclei, and sown with protoplasmic granules is the protoplasmic zone; the other the substance of which forms in its definite arrangement the striated contractile substance, is the muscular zone of the element. This separation of the protoplasmic substance into two distinct regions of the same cell exists in the early stages of the development of all striated muscle, in the higher vertebrates as well as in the amphibia and birds; but it is transitory and purely embryonic. In the heart of certain animals, on the contrary, especially ruminants, this same form, known as the fibers of Purkinge, is persistent in the adult state and serves to trace the union between ordinary and cardiac muscle."

Comparison of the Adult Structure in Man and Certain Other Mammals.

(The scale lines with the figures are 100dths. mm. The full black lines represent the cell-cement.)

In making these comparisons I will begin with man as a standard.

As was stated by Kölliker and also by Weismann, the human cardiac muscle has no sarcolemma, the fibers are more slender, there is a granular appearance, and the fibers

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possess multitudinous anastomoses of varying size and shape. Even Kölliker recognized that the cell is the histological element of the cardiac muscle, but he held that in the adult they were fused with one another. Weismann maintained the same thing, but in another place, as spoken of above, he mentioned the lines of connecting cement, yet as if they were merely traces of original cell divisions. In fact he says: "The original cells are recognizable only by signs." On the other hand Ranvier seems to imply that he had no difficulty in separating *any* of these cells. In my work on the adult human cardiac muscle I have found the truth to lie between these two extremes, some cells readily separating, others not separating but showing the lines of cement, and yet others in which the lines, if present, were not visible, although one would have expected them to appear Fig. I., 3. I found the above to be true in each of the three specimens of human cardiac muscle examined. Frequently fatty granules were found in the cell substance, as mentioned by Kölliker, and which are without doubt identical with the "granular appearance" noted by Weismann. The tran-striæ were very distinct, but the the longi-striæ were not noticeable until acted upon by some reagent as chromic or nitric acid (M. 3, 4). Each cell had one nucleus in the interior of its substance. In but a single instance did I observe two nuclei in the same human cardiac cell (Fig. I., 4.)

The human cardiac cells are, as compared with the size of the body, smaller than in any other animal examined (see table at the end of heading). Owing to their position within the substance of the cell, the nuclei frequently did not appear until acted upon by a reagent.

The lowest form I have been able to examine is the oppossum, and in several particulars its cells are strikingly different from those of man. In all the cells where nuclei were observed they were two in number (Fig. II., 2), and they were larger compared with the size of the cell than in any other animal examined. The fibers also presented a very

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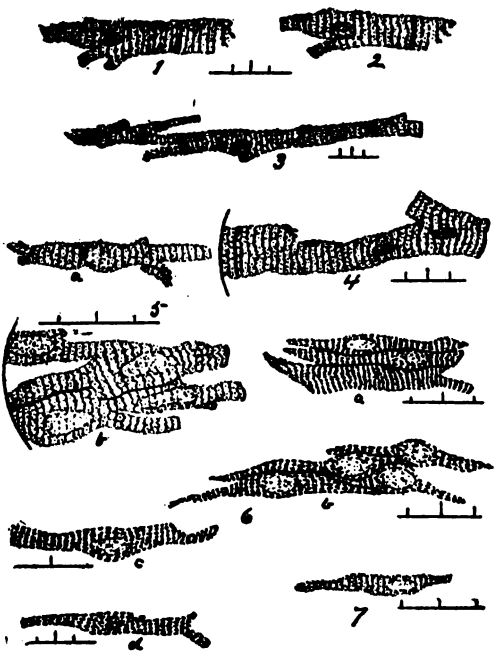

FIG. I. MAN.

Fig. 1.—*Man.* 1, 2 and 3, are from the heart of a man 33 years old. All these show the branchings as seen in man, and that many cell divisions were clearly visible. 3 shows also that these cell divisions are not always visible where one would expect to find them. These were isolated by *Method* 1. 1 and 2 remained in the liquid ½ hour; 3, 1 hour. 4 is from the heart of an old man. It shows that, even in old age, the cell divisions are present. The two nuclei here present in one cell is the only case noticed by me in human cardiac muscle. This was obtained by *Method 2.* 5 is a child at term. It shows that the branches are numerous at this stage. The nuclei are larger than in the adult, but smaller than at six months (see table on p. 295). This was obtained by *Method 1.* 6 is of an embryo at six months. It shows the tran- and longi-striæ about equally prominent. Here the nuclei are at their maximum size (see table on p. 295). This was obtained by *Method 1.* 7 is of an embryo at four months. It shows only tran-striation. The nucleus has not attained its maximum size. There are no branches. This was obtained by *Method 1.*

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striking appearance, having frequent, but not regularly placed, conctrictions,—an appearance as if they had been tied about with cords, and which gave them a scalloped outline (Fig. II.)

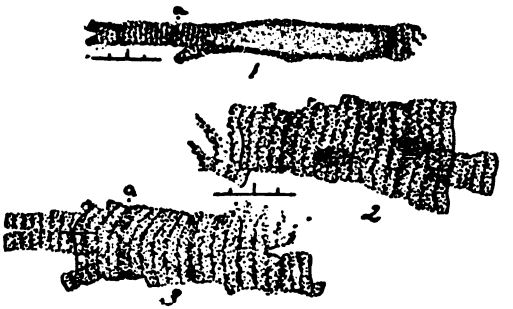

FIG. II. OPPOSSUM.

Fig. II.—*Oppossum.* This was an adult animal. 1 was prepared by *Method 3*, and mounted in carmine-glycerine. 2 and 3 were prepared by *Method 1*. 3 shows something of the numerous branches found in the animal's heart fibers. 1 shows an optical section of a fiber, which, by focussing, was seen to possess striæ above and below, showing that it was a cylinder of striated substance and granular protoplasm within. 1 and 3 show constrictions which were very abundant on the fibers.

Then the number of anastomosing branches is greater than in the human cordiac muscle. But what is even more striking is an appearance as if the fibers were hollow cylinders, and in this respect they present a persistent embryonic condition comparable with that found in the cells of Purkinge. It needs, however, more careful study than the time now at my disposal will permit, in order to reach the full meaning of this appearance. Of course until that has been done any conclusions would be premature.

Among the rodents, in the squirrel and mouse, two nuclei are nearly always present in the cardiac cell, but they are not so large relatively as in the oppossum. The branchings are still present although not so strikingly abundant. (Figs. III., IV.)

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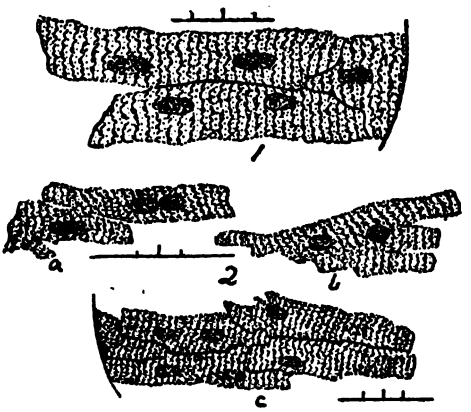

FIG. III. SQUIRREL.

Fig. III.—*Squirrel.* All these drawings were made from potassium preparations. 1 was in the liquid 48 hours and all of 2 from 12 to 16 hours. Two nuclei are quite constant. 2, a, shows the nuclei in a very interesting position (see p. 288). 2, c, shows the relation of neighboring cells.

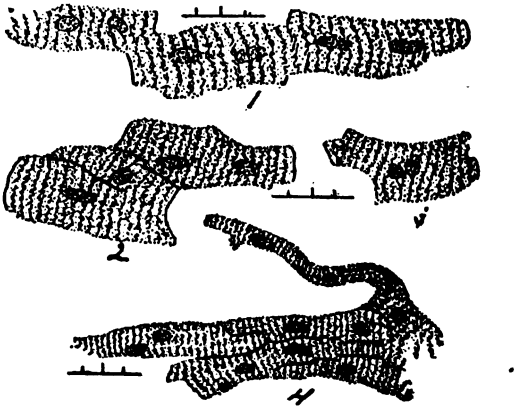

FIG. IV. MOUSE.

Fig. IV.—*Mouse.* These were all drawn from potassium preparations. Here, as in the squirrel, the two nuclei are quite constant. Compare 3 with Plate VI, 2, a. and see p. 288. 4 shows a very interesting relation of cells.

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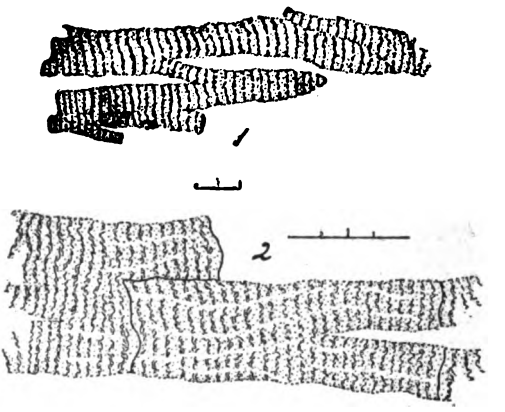

FIG. V. COW.

Fig. V.—*Cow.* The preparations for this figure were from the heart of an *old* cow. 1 was prepared by *Method 1*, 2, by *Method 4*. The cells are very large (see table on p. 292). The nuclei, in many cases, were not distinct, and the cell divisions were not so noticeable as in the cat or dog. 1 shows peculiar cell endings which resemble those found in amphibians and reptiles.

Among the ruminants, as for example the ox, the cell divisions were not so distinct as in the lower forms, or as in some of the higher ones (compare Fig. III., IV. and V.) The animal whose heart was examined was old, and perhaps this may in part account for this peculiarity. I found one condition in this heart which I have not found anywhere else. There were two cells, each of which had an ovately conical end, in place of the usual either abrupt or branching end. This recalls the persistent condition of the cardiac cell in amphibians and reptiles. In the ox the cells were absolutely, but not relatively, larger than in any of the other animals examined (Fig. V., and table at close of this heading).

Among the carnivora, as for instance in the dog, the cells were quite distinct and easily separable in potassium hydrate. Two nuclei still seem to be the rule. In the cat the structure is very similiar to that of the dog, except that there are more branches (Figs. VI., VII).

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Among primates, in the monkey, the cardiac cells are also very frequently doubly nucleated, and they are far more easily separable in potassium hydrate than is the case with man. The nuclei, relatively to the size of the cell, are smaller than in man, although the cells themselves are proportionally much larger, being absolutely about one-third larger, while the body is only about one-twentieth of that of man.

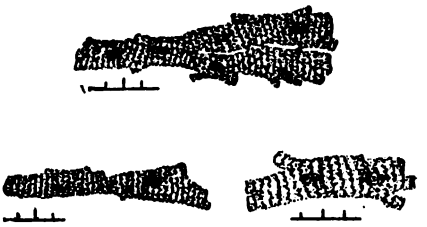

FIG. VI. CAT.

Fig. VI.—*Cat.* These are from potassium preparations which had been in the liquid about 1 hour. They show an increase in branchings over those of monkey. The presence of two nuclei is also more frequent. The animal was adult.

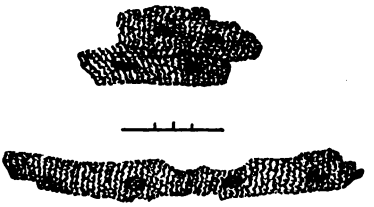

FIG. VII. DOG.

Fig. VII.—*Dog.* Potassium was used in making the preparations from which these drawings were taken. It was an adult terrier dog. It shows a structure very similiar to that of the cat, except that there were not so many branchings.

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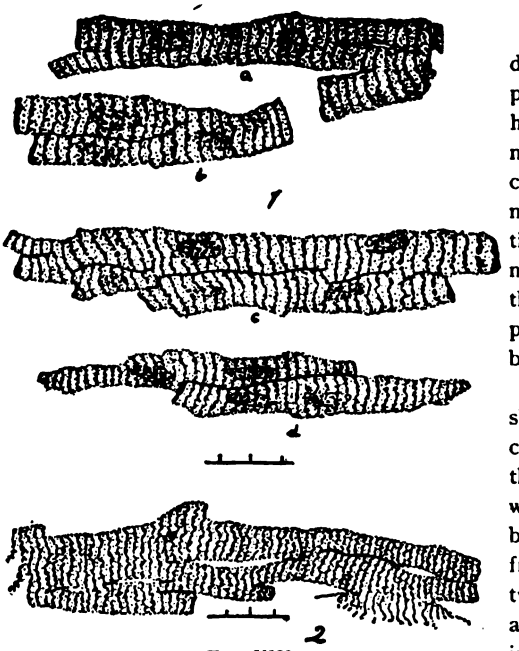

FIG. VIII.

FIG. VIII.——These drawings are from preparations of the heart of an adult monkey. Those included under 1 were made from preparations isolated by means of *Method* 3, those under 2 from preparations isolated by means of *Method* 3.

The drawings show the enormous increase in the size of the cell as compared with man (see table below), and the great frequency with which two nuclei occur. The absence of nuclei in 2 is due to the method of preparation. 1, c, is a good illustration of the manner in which neighboring fibers anastomose.

A Table of Comparisons of the Average Adult Size of Cells and Nuclei.

	CELL.		NUCLEUS.		
NAME.	AVERAGE LENGTH.	NUMBER AVERAGED	LONG DIAMETER.	SHORT DIAMETER.	NUMBER AVERAGED
Man	69.9μ.	5	11.4μ.	5.2μ.	9
Monkey	99 6μ.	5	22.9μ.	4.8μ.	14
Cat	70.6μ.	5	9.6μ.	4.2μ.	6
Dog.....	87.9μ.	4	10.2μ.	4.8μ.	8
Ox	114.5μ.	5	14.5μ.	6.7μ.	2
Squirrel	90.4μ.	5	10.9μ.	5.4μ.	10
Mouse	69.4μ.	5	11.1μ.	5.5μ.	10
Oppossum..	106.8μ.	4	21.4μ.	10.8μ.	5

NOTE.—The object of the above table, as well as the one at the close of the following heading, is to give a truer comparison by eliminating as much as possible the individual variations. It seemed best

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not to make an average of the cell diameter, as in this case the individual variation was such that no uniform place of measurement could be obtained ; and therefore any results thus obtained would be misleading rather than helpful.

Comparison of the Structure in Man at Different Stages of Development.

I have not succeeded in obtaining any very young human embryos. But Foster and Balfour say (1.269) that in the early stages of development the cells are spherical or oblong and gradually become fusiform.

The youngest specimen examined by me was that of an embryo at about four months. In this the cells were about forty microns in length and fusiform, but with some irregularity of outline (Fig. I., 7). There were tran-striæ quite distinctly visible ; but no longi-striæ were observed. In this Weismann concurs, as was above mentioned (p. 284). The nuclei were more transparent than the surrounding substance.

The next example studied was that of an embryo at about six months. In this the cells were of greater size, and the nuclei were relatively larger. They were so large as to cause the surrounding part of the cell to bulge outward ; and the overlapping ends of neighboring cells were indented (Fig. I., 6, b, c).

Here in many cases the tapering ends of the cells showed enlargements and, in some cases indications of the adult anastomosing condition of the cells. Here also longi-striæ had become visible, and it was difficult to determine whether these or the tran-striæ were the more prominent. In all the preparations of embryonic cardiac muscle examined the cell outlines were very distinct (Fig. I., 6-7), and there was not the slightest indication of fusion.

The next specimen examined was that of a child at term. In this the nuclei were still large compared with those of the adult, but smaller than those in the preceding specimen ; the branchings, indications of which were seen in the

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embryo at six months, had been effected (Fig. I., 5); transtriæ had become more prominent again; the length of the cells had increased to between fifty and sixty microns; and the nuclei still appeared more transparent than the surrounding substance.

I have, as yet, been unable to obtain any specimens of adolescent human cardiac muscle, but judging from the difference which exists, between the child at term and the adult, these stages must display many interesting and instructive features.

In the adult the size of the cell had increased while that of the nucleus had decreased and no longer appeared as a clearer portion of the substance, but as a more dense one. The branchings had become more numerous; and whether the cells had amalgamated or not, they were less readily separable in the reagents at present in use. In some cases the lines of cement did not appear where one would have expected to find them (Fig. I., 3). I have not, however, found, as regards the human cardiac muscle, that the cells are less readily separable in the old than in the middle aged. There seemed, also, to be as many cell divisions visible. The only case of a doubly nucleated cell which I have observed in man, was found in the cardiac muscle of an elderly person.

Weismann says (6.55): "The mammalian heart shows first the structure of the mollusk, then of the fish as it passes through the different stages of development." It would also seem natural that the human cardiac cell in the course of its development, should show the structure which is found in the adult lower forms, yet in none of the embryos examined did I find more than one nucleus in a cell. It would, therefore, seem that these double nuclei, so common in most mammals, are the result of a later segmentation of the nucleus or of the amalgamation of cells — probably the former. One fact which would seem to indicate this, in addition to those presented in the historical sketch, is that

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if the cell, or if you prefer it, the segment, which contains two nuclei was the representative of two cells, it would make these resulting cells too small to maintain the ratio with those which have but one nucleus in the earlier stages of development. Then, there are some cases in which the nuclei are not entirely separate or are very close together, and smaller than usual, which would make it appear unlikely that these nuclei represent two originally distinct cells (Figs. III., 2, a; IV., 3).

A Table of Comparison of the Average Size at different Stages of Development.

Human Embryo.	Cell.		Nucleus.		
Age.	Average Length.	Number Averaged	Long Diameter	Short Diameter	Number Averaged
Four months.....	42. μ	1	7.8μ	2.6μ	1
Six months.....	64.7μ	6	12.9μ	6.8μ	5
Nine months.....	64.3μ	5	12.4μ	5.6μ	7
Adult	69.9μ	5	11.4μ	5.2μ	9

Note.—The apparent absence of growth, from the sixth to the ninth month, may be due to the fact that during this time the cells are expanding in breadth rather than length. (See also note at close of last heading.)

METHODS OF WORK.

I. *Methods of Preparation.*

1. The Potassium Hydrate Method: This was introduced by Moelschott and has been in use ever since.

In a 35 to 40 per cent. (35 to 40 grms. K O H and 65 to 60 c.c. H_2 O) aqueous solution of caustic potash a fragment of muscle is placed for from fifteen minutes to as many hours. A weaker solution dissolves the cells instead of separating them.

It is much more effective if the blood is first soaked out of the portion of muscle to be used, since if this is not done

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the preparation will appear cloudy and the details can not be clearly seen. This is the most effective of any of the methods which I have tried for dissolving the cell cement and so for separating the cells. It does not bring out the longi-striæ, but it does make the nuclei very distinct if it acts for an hour or more.

Permanent preparations may be made, where this solution has been used, as follows: Add to the edge of the cover enough potassium acetate to replace the alkali, then stain; add glycerine and seal as usual.

2. Frozen Sections: This method is, in some respects, better than the preceding, as it leaves the tissue in a more nearly natural condition. But it necessitates having a freezing microtome, and so, in some cases, might not be available.

It consists in taking a small piece of muscle, say a cubic centimeter (not more, as the sections can thus be made thinner than if a larger piece were taken), which is then frozen by either spray or other means as the microtome may require. Care should be taken not to freeze the tissue too hard, since just as much difficulty is experienced in making good sections when it is frozen too hard as when not frozen hard enough. The proper degree of hardness can be easily determined after a little practice. After the sections have been made it is best to float them out in water. They may then be stained and mounted in the ordinary manner.

3. The Nitric Acid Method: This method was devised by Reichart in 1849. It consists in macerating the tissue in weak nitric acid (concentrated $H\,N\,O_3$, 23 parts, $H_2\,O$, 77 parts) for about three days. The exact time can be determined only by examination, as it varies in different cases. When it is sufficiently macerated, the acid is removed by soaking in water for several hours, after which it may be teased out with needles and stained. This method brings out the longi-striæ very nicely.

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4. Chromic Acid Method: This method is recommended by Ranvier (5. 54) as the best method to demonstrate the cell-cement. A piece of muscle is placed for twelve hours or more in a one-tenth per cent solution of chromic acid; then after washing with water it may be teased out with needles, stained in picro-carmine, and mounted in formic acid one part, to glycerine ninety-nine parts. This brings out both systems of striæ finely as well as the cell cement.

II. *Methods of Staining.*

The stain needed is one to bring out the nuclei and the cell-cement. For the fresh sections, obtained with the freezing microtome, I found that ammonia-carmine gave excellent results, but did not acertain whether it is applicable to preparations obtained by means of reagents. With nitric acid preparations, a mixture of glycerine 90 c. c. and alum-carmine 10 c. c. proved very satisfactory, both as a stain and as a mounting medium.

III. *Methods of Observation.*

In this investigation, powers ranging from 320 to 1000 diameters were used, but usually about 500.

IV. *Methods of Drawing.*

In making the drawings I used constantly the Abbe' camera lucida. This has the great advantage of permitting the microscope to remain in an upright position, so that there is no danger of the object moving out of the field of view before the drawing is completed. The measurements were made by means of the stage micrometer, thus avoiding any error from change in the length of the tube, or from any mistake in calculating the power. It simply magnifies the micrometer just as much as, and no more than, it does the object. The only chance for error is in drawing the lines of the micrometer on the paper. By drawing a large number of these lines on the paper and taking their average, the error is reduced to a minimum.

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The permanent drawings were made, in all cases, from those taken under the camera. This was accomplished by tracing, so that the amount of variation, aside from any defect in the use of the camera, could not be more than twice the width of the pencil's point. The filling in of the striæ was freehand and arbitrary. It only aims to produce, as nearly as may be, an impression upon the eye similar to the one made by the object under the microscope. Perhaps this is most nearly realized by holding the drawing at about two feet from the eye. (The drawings were reduced one-half, Ed.)

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COMPARISON OF CENTIMETER SCALE, "FASOLDT II," WITH "CENTIMETER SCALE A."

By Marshall D. Ewell, M. D., F. R. M. S., Chicago, Ill.

In order to distinguish this scale from a similar one owned by this Society and known as "Fasoldt I," I have, with a writing diamond, marked it "Fasoldt II." The comparisons below recorded were made with the same apparatus described in the paper read at the last session (Proceedings Am. Soc. Microscopists, ix, p. 75), using a Bausch & Lomb's three-fourth opaque illuminator and a Bullock-Filar micrometer, one division of which = 0.299μ.

Date.			
July 11,	1887	F. II	2.39μ = A
July 12,	1887	F. II	1.97μ = A
July 13,	1887	F. II	2.99μ = A
July 14,	1887	F. II	3.08μ = A
„	1887	F. II	2.66μ = A
July 15,	1887	F. II	3.11μ = A
„	1887	F. II	3.35μ = A
July 16,	1887	F. II	3.29μ = A
„	1887	F. II	2.60μ = A
July 17,	1887	F. II	3.08μ = A
„	1887	F. II	3.20μ = A
July 18,	1887	F. II	3.80μ = A
July 19,	1887	F. II	3.02μ = A
July 20,	1887	F. II	4.19μ = A
„	1887	F. II	3.62μ = A
July 21,	1887	F. II	3.68μ = A
„	1887	F. II	2.33μ = A

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Date.			
July 22,	1887.....	F. II.....	3.41μ = A
July 23,	1887.....	F. II.....	3.41μ = A
„	1887.....	F. II.....	2.42μ = A

Mean of 20 comparisons —

"Fasoldt II," 3.08μ = "A."

Assuming that "A" — $0.17\mu = \frac{1}{100} A_o$, that being the result arrived at from the observations recorded in the paper read by the writer at the last session, combined with prior observations, we have

Fasoldt II—$3.25\mu = \frac{1}{100} A_o$.

From the mean of the twenty comparisons recorded in the paper read at the last session we found "Fasoldt I"—3.18μ="A." Substituting the value of "A" as above, we have

"FasoldtI"—$3.35\mu = \frac{1}{100} A_o$.

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THE ERYSIPHEÆ OF ILLINOIS.

By T. J. Burrill, Ph.D., Champaign, Ill.

The Erysipheæ are microscopic fungi parasitic upon flowering plants, for the most part occurring on the leaves. They are often called "mildews" or "blights," two popular terms of very diverse application. The family is not a large one, but is well characterized and full of interest to the biologist as well as to the technical microscopist. As beautiful and popularly interesting objects under low or medium magnification nothing of the kind surpasses them, while their peculiar structure and details of life-history invite and tax, to the full capacity of trained skill, the student and investigator. The little parasites are exceedingly common objects, developing especially in late summer and autumn.

Affected leaves, like those of the common lilac, appear as if sprinkled or coated with white or gray dust. The powdery or cobwebby substance varies in all degrees of abundance and conspicuousness. Frequently the coating can be readily rubbed off, leaving the leaf apparently clean and, perhaps, not evidently injured. More often, however, the affected parts of the host plant is conspicuously injured, as shown by the discoloration or distortion of these parts. Not unfrequently the noticed injury is characteristic of the special species. Thus the leaves of the cultivated cherry tree are thickened and made inflexible, and are more or less curled under the influence of *Podosphaera oxyacanthæ*, while affected spots on maple leaves with *Uncinula circinata* retain abnormally their green color late in the season. Most often

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the diseased leaves become brown and die in spots and patches.

The Erysipheæ are peculiar among the leaf parasites, in that they live upon the surface and gain their stolen nutriment by sucker-like organs, called haustoria, which fit closely to the epidermis, and take through this the elaborated juices of the host by imbibition. The vegetative portion of the fungus, constituting the main part of the whitish material above mentioned, and known as the mycelium, consists of slender, branched and interlaced threads which never unite into a tissue. When fresh and growing the creeping threads resemble those of a spider's web ; when old and dry the whole mass may be easily broken up into dust.

The haustoria or suckers are short branches of the vegetative threads, and what are called asexual spores or conidia are mere joints of other modified branches. These latter arise from the creeping filaments; a cross partition forms near the apex of the young erect branch, and after a little further growth another partition, just below the first, cuts off a second cell, then a third cell is similarly formed, and a fourth, a fifth, etc., each new one below the older ones, until at length there exists an erect necklace-like row or string of pearly-white oval cells or spores. (Fig. 3, c.) These readily separate from each other and may be borne as dust in the wind. Their vitality is retained but a short time—at most only a few days. Falling upon the proper host, under the requisite conditions of moisture and temperature, they quickly germinate and reproduce the mycelium of a new plant.

The moniliform rows of spores (conidia) usually stands closely together like a miniature forest. Taken at this stage of development a bit of the affected leaf, as an opaque object under a half-inch objective and binocular tube, is an instructive and charming object. To appreciate what art can do in the pictorial representation of such an object, Tulasne's *Selecta Fungorum Carpologia* should be consulted.

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At a later time in the history of the parasite a kind of fruit is produced totally unlike that just described. It is much more complicated in its structure and course of development. A process of fertilization or sexual union gives it origin as follows: A short branch of the mycelium swells to two or three times the ordinary diameter, forming an oval-shaped organ called the carpogonium, homologous with the pistil of flowering plants. From a neighboring mycelium thread another short branch, which remains slender, is sent forth and becomes the antheridium, representing a stamen of a flower. (Fig. 1, d.) This reaches the carpogonium and grows to its summit, over which the end is bent. An apical cell is separated by a partition, and at length the contents of this cell is transferred into that of the carpogonium effecting its fertilization. After this the body developes into a little sphere, with a compact, firm, cellular wall, within which are found one to several transparent sacks (each called an ascus — plural, asci), each containing two to many oval white spores. The number of asci and of spores are characteristic of genera and species. The mature spheres, named perithecia, are dark, reddish brown, or almost black, and just large enough to be seen by good unaided eyes. (Figs. 3–8.) When young they are yellow. They may be readily dissected for study by gently crushing them in water under an ordinary cover glass, pressure being made with a mounted needle, or, often better (before placing slip on stage), with the rubber at the end of a lead pencil.

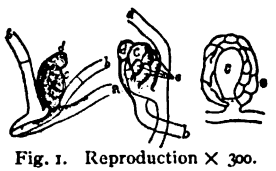

Fig. 1. Reproduction × 300. (After De Bary).

The perithecia with their contained reproductive bodies, the spores, are destined to remain unchanged over winter, the sole surviving part of the parasitic plant. At the proper time the succeeding season the previously firm walls of the little spheres become decomposed, the spores are liberated from their prison, and being scattered by the wind, some of them fall upon their special hosts and, germinating, reproduce their kind.

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One more set of structures remains to be described. Radiating from the perithecia are found thread-like organs called appendages. These vary in every way according to the species, but are always more or less distinct from the mycelium, usually of thicker diameter, firmer texture and more durable. They arise from the perithecium itself and the ends are free. In one genus these latter are simple and straight, in an other circinately coiled, in others dichotomously divided and curled in an exceedingly complicated and ornate manner. The use of these curious organs is not clearly perceived. Indeed they do not seem to be of any service, unless it is that they hold the little fruit balls in place by becoming entangled with the mycelium, a very doubtful idea. If they really have no serviceable office to fill for the plant, what explanation can the students of evolution give for their existence at all, or, in any event, why the exceedingly beautiful forms, rivaling the special ornamental appendages of animals, supposed to have been developed by sexual selection? Surely this latter principle does not apply to these fungi.

Very often there may be found in connection with the two kinds of fruits now described, a third, consisting of erect, usually somewhat flask-shape, bodies filled with simple, small spores. These organs have been supposed to be true fruits of the leaf parasite and the spores serviceable for the rapid distribution of the fungus, after the manner of the conidia above described. Certain it is the flask-like bodies arise from the same mycelium as do the conidia and perithecia. The name pycnidia is applied to them. But the acute German botanist, DeBary shows (Morph. u. Phys. d. Pilze III., pp. 53–75, Tafeln VI., VII.) that they are fructifications of a parasite dwelling inside the cells of the fungi upon which we write. This is a capital point. One who wants to verify it and, at the same time, find an object to test his skill in handling good objectives, may especially look at specimens of *Erysiphe cichoracearum*, or, as it is more com-

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monly known, *E. lamprocarpa*, often occurring on leaves of Helianthi and Verbenæ. The accompanying figure will aid this description. Some times little spheres, exactly resembling the perithecia exteriorly, are found filled with free spores altogether similar to those of the pseudo-pycnidia. These have been accounted a fourth form of reproductive organs. (Berkley, Introduction Crypt. Bot. p. 78.) Though satisfactory proofs have not been made, it is probably also a case of parasitism, the same as just described, except as to the parts affected. DeBary names the parasite upon the parasite *Cicinobolus Cesatii*.

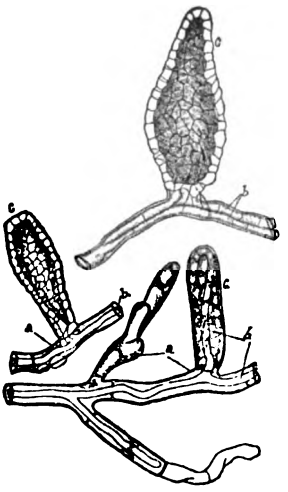

Fig. 2, Cicinobolus. ×300. (After DeBary.)

The species of Erysipheæ affect special host plants, but many of them have a far wider range in this respect than most other parasitic fungi. The classification and identification of the species is based for the most part upon morphological characteristics, and there is a chance for acute discrimination in this matter. Many names have been given, with descriptions of supposed species, which, upon closer study, must be recognized as only slightly varying forms of previously described species. In writing up the Erysipheæ of Illinois much study of numerous specimens gathered from all quarters of the world has been given to this particular by Mr. F. S. Earle and myself. The results are summarily seen in the list of names and synonyms given herewith.

It will be observed that many forms considered as species by various authors are here included under one name,—always the first published. For the European species this condensation had already been made by Winter in his admirable *Die Pilze, Deutschlands Oesterreichs und der Schweiz*, and his work has been followed, so far as applicable, in the

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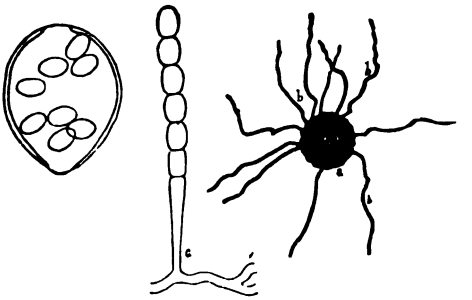

Fig. 3. Sphærotheca. ×250.

Fig. 4. Erysiphe. ×90.

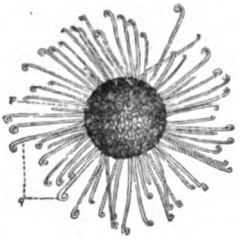

Fig. 5. Uncinula. × b 100, c and d 200.

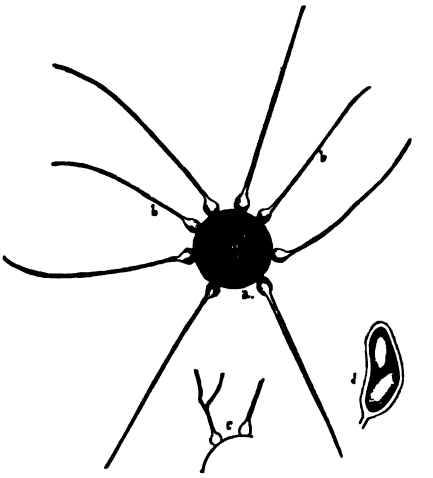

Fig. 6. Phyllactinia. ×a 50, d 100.

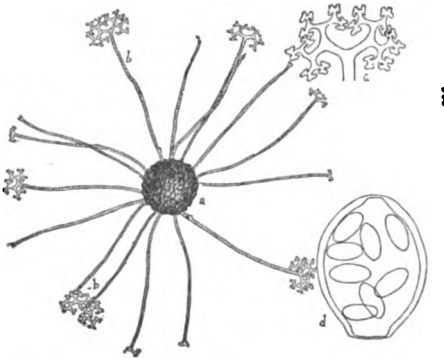

Fig. 7. Podosphera. ×a 90, c & d more.

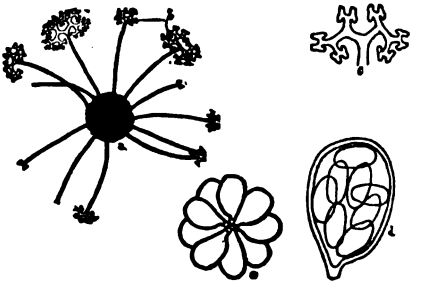

Fig. 8. Microsphæra. ×a 75, c and d more.

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present paper. It remains for others to say how acceptable this may be. No new species have been described. The one on Catalapa was published by myself several years ago.

Lévillé's classification of genera is adopted and made intelligible by the following key and illustrations, the cuts for the latter being kindly loaned by the Director of the Illinois State Laboratory of Natural History.

Key to the Genera of Erysipheæ.

I. Appendages simple threads similar to the mycelium .....A.
II. Appendages conspicuously different from the mycelium .....B.
A. Perithecium containing one ascus .....SPHÆROTHECA.
Perithecium containing several asci .....ERYSIPHE.
B Appendages simple (not forked) .....1.
Appendages dichotomously forked at tip.....2.
1. Appendages coiled at tip, asci several .....UNCINULA.
1. Appendages needle-shaped, asci several.....PHYLLACTINIA.
2. Perithecium containing one ascus .....PODOSPHÆRA.
2. Peritheceum containing several asci.....MICROSPHÆRA.

LIST OF ILLINOIS SPECIES.

Spætrotheca pannosa, (Wallr) Lév.
Alphitomorpha pannosa, Wallr.
Erysibe pannosa, Link.
Eurotium rosarum, Grev.
On roses.

S. mors-uvæ, (Schw.) B. C.
Erysiphe morsuvæ, Schw.
On gooseberry and currant.

S. pruinosa C. and P.
On Rhus copallina.

S. humuli, (DC) Burrill.
Erpsiphe humuli, DC.
Sphærotheca, castagnei Lev., in part.
On Agrimonia Eupatoria.

The species are fully described in a recently-issued Bulletin of the Illinois State Laboratory of Natural History, from which this paper is largely derived. Mr. F. S. Earle must be credited with much of the preparation.

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S. Castagnei, Lév.

On various Compositæ and species of Veronica, Gerardia and Brunella.

Erysiphe liriodendri, Schw.

On Liriodendron Tulipifera.

E. communis, (Wallr.) Fr.

Alphitomorpha communis. Wallr. A. horridula, Wallr., in part.
Erysibe cummunis, Rabh, E. nitida, Rabh. and E. horridula, Rabh., in part.
Erysibe communis, Link, in part.
Erysiphe aquilegiæ, DC.
Erysiphe pisi, DC.
Erysiphe convolvuli, DC.
Erysiphe polygoni, DC.
Erysiphe Martii, Lév.

On many different hosts, especially upon plants belonging to the Ranunculaceæ and Legunimosæ.

E. galeopsidis, DC.

E. lamprocarpa, Lév., in parts.
E. labiatorum, chev.

On various Labiateæ.

E. cichoracearum, DC.

Alphitomorpha communis, Wallr and A. horridula, Wallr., in part.
Alphitomorpha lamprocarpa, Schl.
Erysibe communis, lamprocarpa, depressa, horridula, Link and Rabh. in part.
Erysiphe horridula, Montagnei, and lamprocarpa. Lév., in part.
Erysiphe ambrosiæ, verbenæ, phlogis, asterum, Schw.

On many and widely different hosts, especially species of Compositæ.

Uncinula ampelopsidis, Peck.

U. Americana, Howe.
U. Spiralis, B. & C.
U. subfusca, B. & C.

On grape and Virginia creeper.

U. macrospora, Peck.

On elm.

U. flexuosa, Peck.

On buckeye.

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U. circinata, C. & P.

On maple.

U. parvula, C. & P.

On hackberry.

U. salicis, (DC.) Winter.

Erysiphe salicis, DC.
Erysiphe populi, DC.
Alphitomorpha adunca, and A. guttata, Wallr.
Erysibe adunca and E. obtusata, Link.
Erysiphe aduuca, Grev.
Uncinula adunca, Lév.
Uncinula leuculenta, Howe.
Uncinula heliciformis, Howe.

On willows and poplars.

Phyllactinia suffulta, (Rebent) Sacc.

Sclerotium suffultum, Rebent.
Erysiphe coryli and E. fraxini, DC.
Erysiphe vagans, Bivon.
Alphitomorpha guttata, Wallr.
Erysibe guttata, Link.
Erysibe guttata, Fries.
Phyllactinea guttata, Lév.

On many widely different hosts, always trees and shrubs.

Podosphæra oxyacanthæ, (DC.) DeBary.

Erysiphe oxyacanthæ, DC.
Alphitomorpha tridactyla, and A. clandestina, Wallr.
Erysibe trydactyla, Rabh.
Erysibe clandestina, Link.
Podosphæra, Kunzei, and P. clandestina, Lév.
Podosphæra trydactyla, and P. myrtillina, DeBary.
Podosphæra myrtillina, Kunze.
Podosphæra minor, Howe.

On various hosts, mostly Rosaceæ.

Microsphæra semitosta, B. & C.

On button bush.

M. Russellii, Clinton.

On Oxalis wood sorrel.

M. diffusa, C. & P.

On several Leguminosæ.

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M symphoricarpi, Howe.

On Symphoricarpus species, snowberry.

M. vaccinii, C. & P.

On huckleberry and cranberry.

M. euphorbiæ, B. & C.

On species of Euphorbia.

M. Erinophila, Peck.

On the "erineum" (Phytoptus galls) of beech.

M. Ravenelii, Berk.

On honey locust and other Legunimosæ.

M. Alni, (DC.) Winter.

Erysiphe alni and E. betulæ, DC.
Alphitomorpha penicillatum, Wallr.
Erysibe penicillata, Link.
Erysiphe viburni, Duby.
Erysiphe ceanothi, E. viburni, and E. syringæ, Schw.
Microsphæra Hedwigii, M. penicillata, and M. Friesii, Lév.
Microsphæra platani, Howe.
Microsphæra Van Bruntiana, Gerarde.
Microsphæra viburni, Howe.
Microsphæra pulchra, C. & P.

On many different hosts,—Ceanotaus, Euonymus, Viburnum, Ulmus, Syringa, (lilac), Platanus, Carya, etc.

M. quercina, (Schw.) Burrill.

Erysiphe quercinum, Schw.
Microsphæra extensa, C. & P.
Microsphæra abbreviata, Peck.

On oaks.

M. elevata, Burrill.

On Catalpa.

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NOTE ON MICROSCOPICAL EXHIBITIONS.

By R. H. Ward, M. D., F. R. M. S., Troy, N. Y.

As there seems to be still some difference of opinion in regard to the policy of changing slides during an evening spent at a microscopical exhibition, it may be worth while to add a few words on the subject, as supplemental to my note of last year on Microscopical Societies.

It cannot be denied that popular exhibitions are very generally tolerated, if not approved, by microscopists, and pleasing to their friends, and that they will continue to be held without regard to any lofty criticisms we may be tempted to make as to their low scientific character or various minor faults. Nor does it seem more doubtful, to the writer, that they can be made, and often are made, not only reasonably entertaining, but also practically efficient in cultivating a scientific taste and diffusing at least a little of scientific knowledge. If this be true it only remains to accept them as a means for good, and to make of them the most and the best that the somewhat difficult circumstances allow.

It will scarcely be questioned that if an intelligent visitor, however little versed in science, could sit down for an hour with a competent microscopist and spend the time in examining and discussing any tolerably good slide that might happen to be under the microscope, the hour would be profitably as well as pleasantly employed. Nor is there any reason why less time similarly spent should be wholly unsatisfactory. The question of the microscopical exhibition, then, assuming only that the exhibitors are fairly com-

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petent and the visitors moderately intelligent, resolves itself into a mere problem of time; is there time enough, as such affairs are managed or could be managed, to make them either creditable amusement or instructive lessons, or both?

Assuming three hours, from 8 to 11 o'clock, to be the maximum time devoted to the purpose, and the objects to remain unchanged during that time, it is evident that if only sixty persons undertook to see any one object, and no time was wasted meanwhile, each of the persons would have three minutes to spend upon each object. But sixty persons would constitute a class or a select party rather than a public one, and if only a hundred and eighty persons were to be provided for, which is still far below a reasonable estimate, then each person could spend upon each object one minute only; and only forty seconds within the more probable limit of two hours instead of three. Even this might still be a respectable performance, as it would afford opportunity for a leisurely glance at the objects if not for the exchange of a few thoughtful words about it. But it would be preposterous to attempt to improve matters, already too crowded, by changing objects every hour, and thus reducing the individual allowance to twenty seconds, or so much of that as might not be lost in transferring from table to table, or exchanging a word with some passing friend. Such needless cutting up of the time, by trying to do too much, does make a bigger program and a longer list of objects, but it also takes away all chance for thought or pleasure, and converts the meeting into a senseless march from stand to stand with no apparent hope or purpose except to be able to say "oh, my," to each object.

All this is upon the assumption that the hall occupied is so large in proportion to the party that there is an abundance of spare room, and that no time is lost in the selection of objects or in waiting for other observers, and none spent at rest or in social intercourse; conditions which are never even

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approximately true, as whenever there are more visitors than microscopes somebody must be waiting.

The absurdity of attempting more than can possibly be done reaches its climax when a party of 2,000 or 3,000 is invited to enjoy an exhibition in which the objects are to be changed every hour. Wandering about the room at will to inspect certain selected exhibits being impracticable, as indeed it is in much smaller parties, one must be content to drift with the crowd and take his turn at the instruments in regular succession, and must be satisfied with a stay of one or two seconds at each object, less that wasted in waiting or otherwise employed. This comedy, which is more amusing than satisfactory to the fortunate ones who have glanced at a few of the slides, dwindles to a farce to that large proportion of the company who have not yet seen their first object, and who stand in long lines waiting with manifest delight to see the exhibitors solemnly spending five or ten minutes in changing, at the tap of the bell, objects which they have not yet seen for others which, not unlikely, may be changed again before they get within reach of them. The variety secured by changing objects because they have been seen by somebody else appears to be the one redeeming feature of this somewhat showy plan.

From every point of view it seems that the time offered by an evening exhibition is little enough for a small party; and that the one unavoidable fault of a large company, the want of adequate time, can only be absurdly increased by an attempt to change the objects.

A singular practical mistake has been made at many exhibitions by arranging a convenient line of march around the room, and then inadvertently attempting to apply, to the opening of the exhibition, the rule enforcing this course. So long as this order is maintained the visitors must enter slowly and in single file, in order to begin their observations at table number one, while the room beyond is quite unoccupied and the time of most of the party is being thrown

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away. Sooner or later the reasonable impatience of the crowd breaks over restraint and insists on making a way directly to the unoccupied tables, where they ought to have been sent to begin with. Obviously when the time for opening arrives, and the exhibitors are ready, the crowd should be abmitted to the whole floor, so that observations may begin at all the microscopes at once; after which the tides of people may be reasonably expected to move only in the specified directions.

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THE CRYSTALLOGRAPHY OF BUTTER AND OTHER FATS.

By Thomas Taylor, M.D., Microscopist U. S. Department of Agriculture, Washington, D. C.

PLATES I. and II.

CRYSTALLINE FORMATIONS OF BUTTER AND FATS.

Figs. 1, 2, 3 and 4. Represent primary crystals of butter, ×80 to 110.
Figs. 5 and 6. Secondary crystals forming within primary crystals.
Figs. 7 and 8. Secondary crystals which have separated from the primary forms. ×80 to 110.
Figs. 9, 10 and 11. Tertiary crystals of butter. ×80 to 140.
Fig. 12. Tertiary passing into the amorphous. ×140.
Figs. 13, 14, 15 and 16. Represent oleomargarine. ×80 to 110.
Fig. 17. Oleo, ×140. This crystal is not found in unboiled oleomargarine.
Fig. 18. Oleo in its second stage, as seen in oleomargarine as sold.
Figs. 19 and 20. Common lard. ×140 to 400.
Figs. 21, 22, 23 and 24. Crystals of beef-fat from various tissues of the ox. (Omentum, kidney, marrow of the femur, and round.)

PLATE III.

CRYSTALLINE FORMATIONS OF BUTTER.

Figs. 1, 2, 3, 6, 8, 9, 12 and 14. Primary crystals of normal butter. ×80 to 110.
Figs. 4, 7 and 10. Primary crystals showing "secondaries" forming.
Figs. 13 and 15. Secondary crystals of butter. ×80 to 140.
Figs. 5 and 11. Tertiary crystals of butter. ×80 to 140.

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PLATE IV.

CRYSTALLINE FORMATIONS OF OLEO AND BUTTER.

FIGS. 1, 2, 4 and 11. Crystals of boiled oleo (Armour). ×70 to 140.

FIGS. 3, 5, 6, 7, 8 and 9. Crystals of boiled oleo in process of decay. Such forms are frequently observed in oleomargarine. ×140.

FIG. 10. The butter crystal as photographed by Detmers.

FIG. 12. A crystal of oleo and lard made by Prof. Weber, which he says cannot be distinguished from that of pure butter. (See Figs. 10 and 14.)

FIGS. 13 and 15. Crystals of boiled butterine as prepared by Prof. Weber and photographed by Prof. Detmers, representing the butter crystal according to Prof. Weber.

FIG. 14. The true butter crystal, photographed by the late Dr. Bernard Persh. Compare this plate with the transition stages of butter crystals, Plate I.

PLATE V.

CRYSTALLINE FORMATIONS OF OLEO AND OLEOMARGARINE.

FIG. 1. Boiled oleo by plain light, exhibiting spines. ×140.

FIG. 3. Boiled oleo by polarized light, showing a cross. ×140.

FIGS. 2, 4, 5, 6, 9, 11, 12, 13, 14 and 15. General appearance of oleomargarine as sold in the market. ×75 to 110.

FIG. 7. Armour's oleomorgarine boiled and cooled. ×140.

FIG. 10. A specimen of oleomargarine composed mostly of stearine and cottonseed oil. ×110.

FIG. 8. Boiled butterine (Armour's make), showing the oleo crystals.

The above crystals were all photographed by polarized light, except in the case of Fig. 1, which was by plain light.

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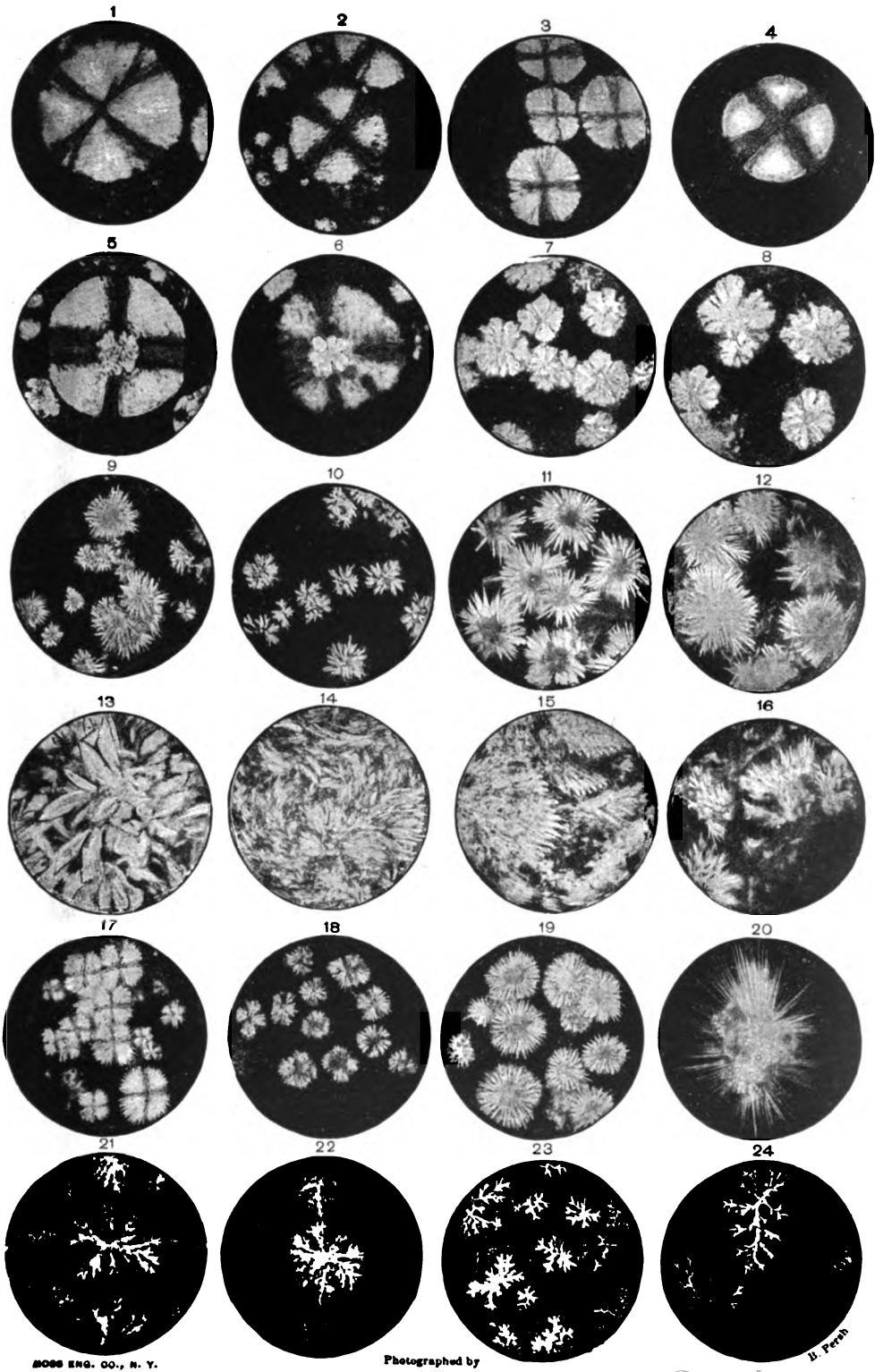
PLATE I.

CRYSTALLINE FORMATIONS OF BUTTER AND FATS.



MOSS ENG. CO., N. Y.

Photographed by
Perah and Gascoyne.

H. Perah

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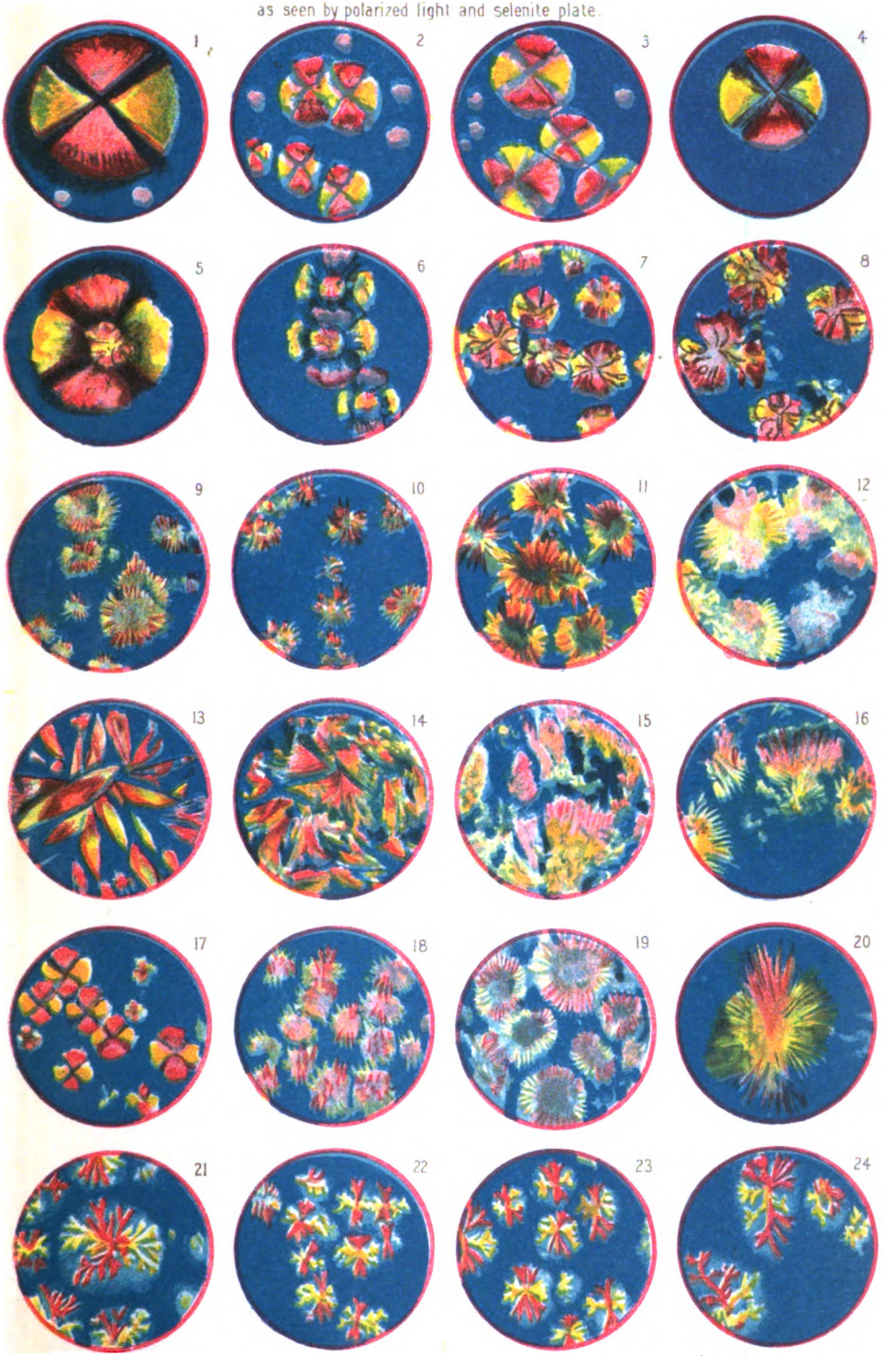
Plate II.

CRYSTALLINE FORMATION OF BUTTER AND FAT.

as seen by polarized light and selenite plate.



T. TAYLOR, del.

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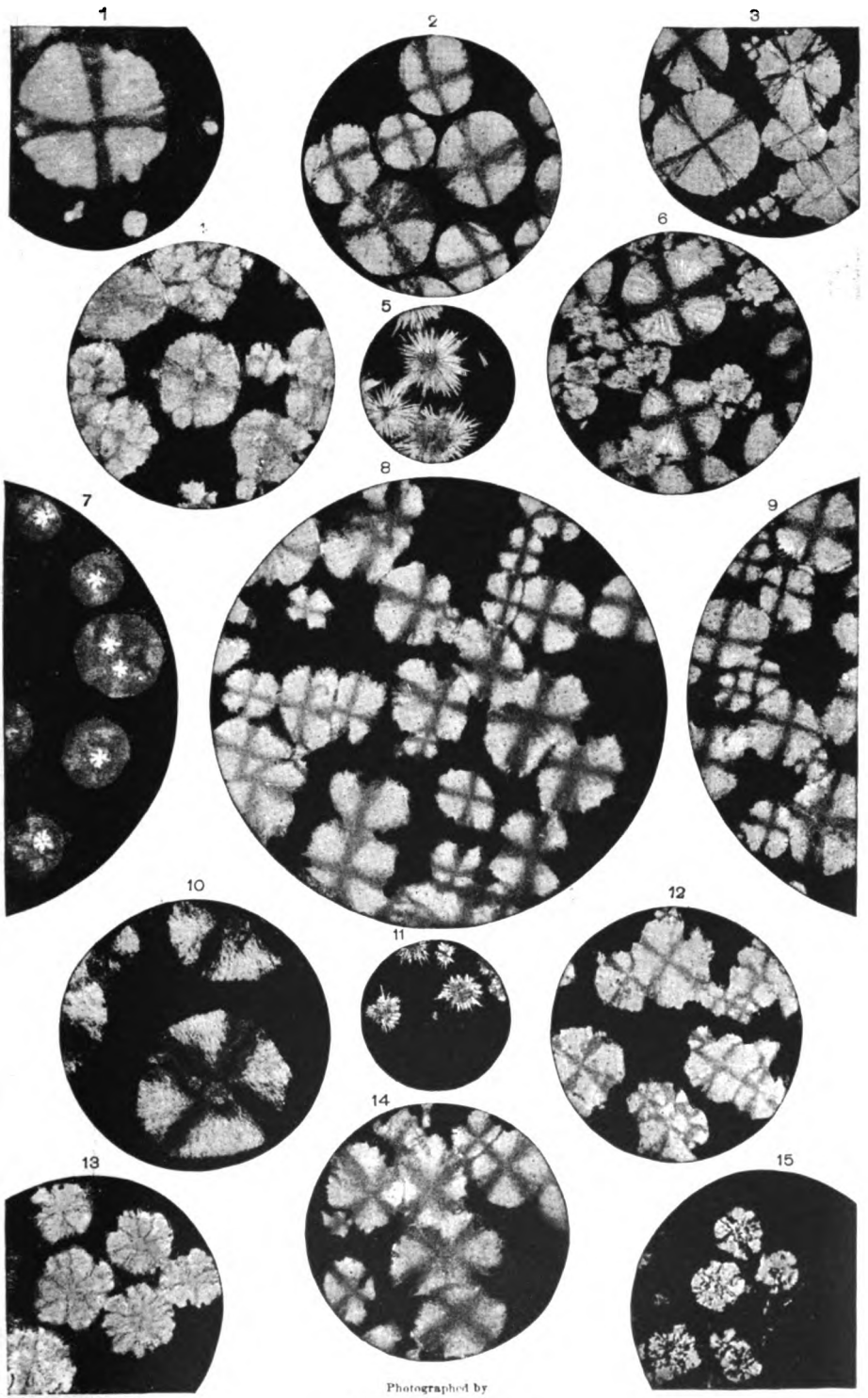
PLATE III.

CRYSTALLINE FORMATIONS OF BUTTER.



Photographed by
Perth, Walmsley and Galloway

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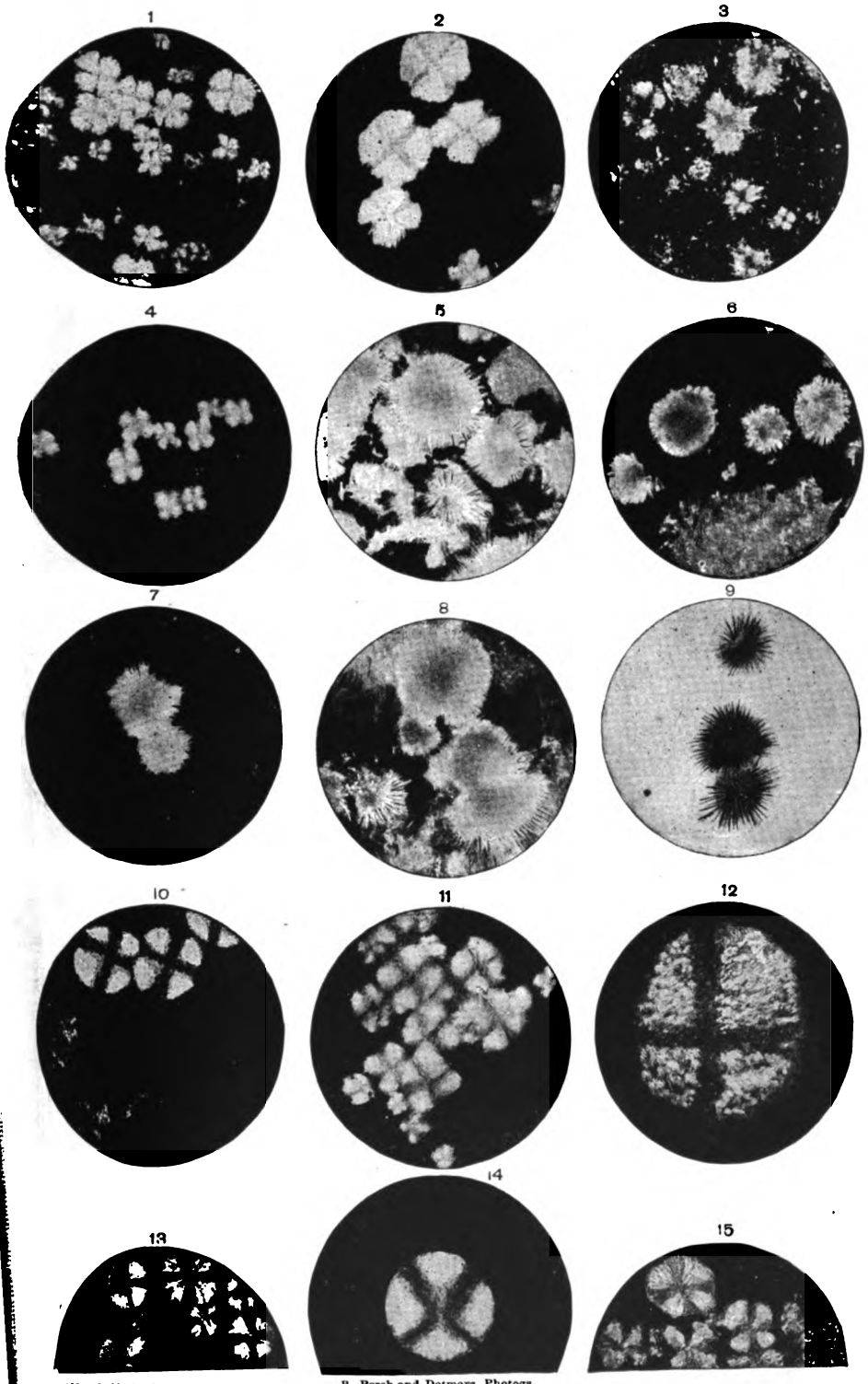
PLATE IV.

CRYSTALLINE FORMATIONS OF "OLEO" & BUTTER.



B. Persh and Detmers, Photogs.

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PLATE V.

CRYSTALLINE FORMATIONS OF "OLEO" & OLEOMARGINE. BOILED AND RAW.

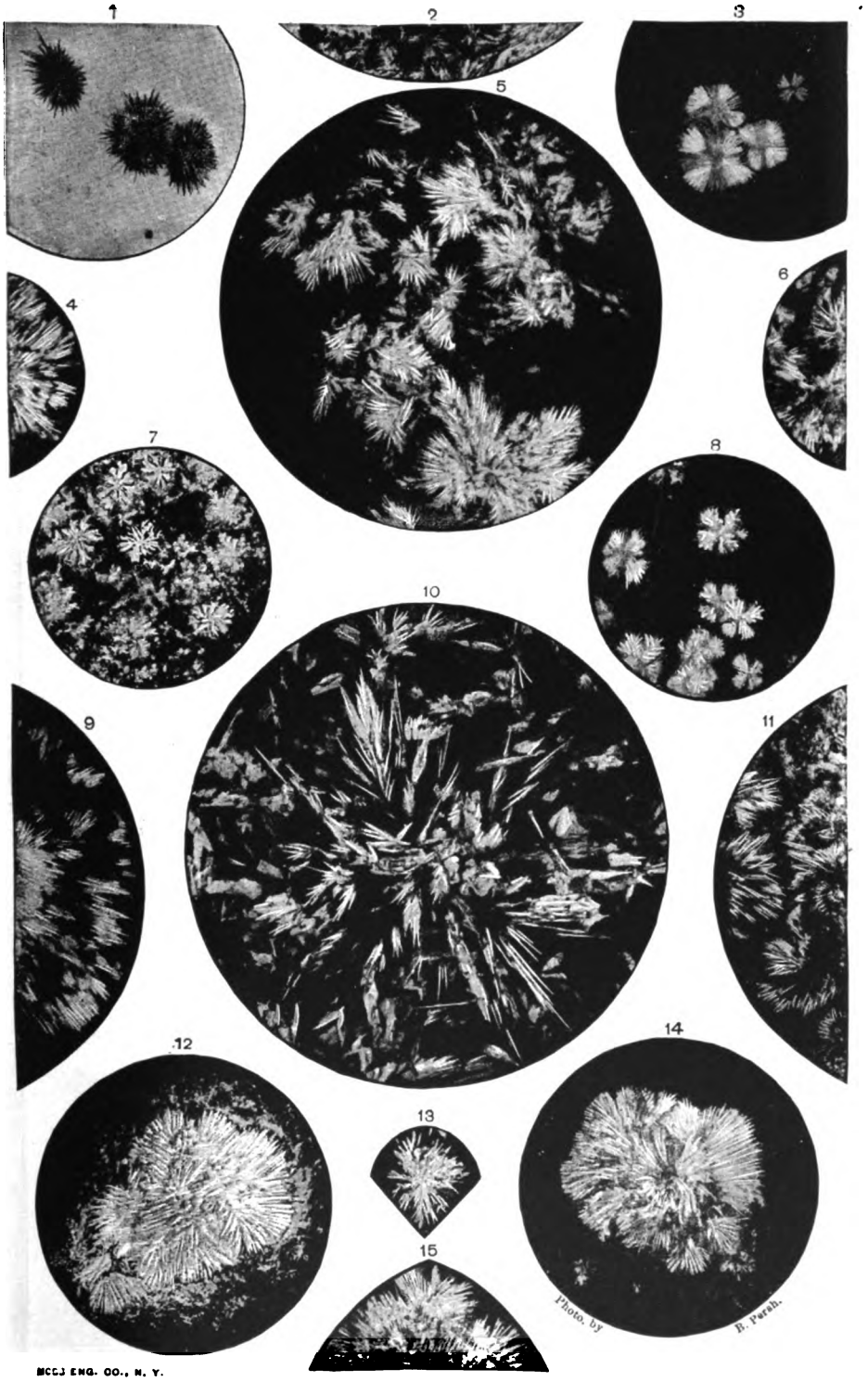

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PLATE VI.

CRYSTALLINE FORMATIONS OF LARD AND OTHER FATS.

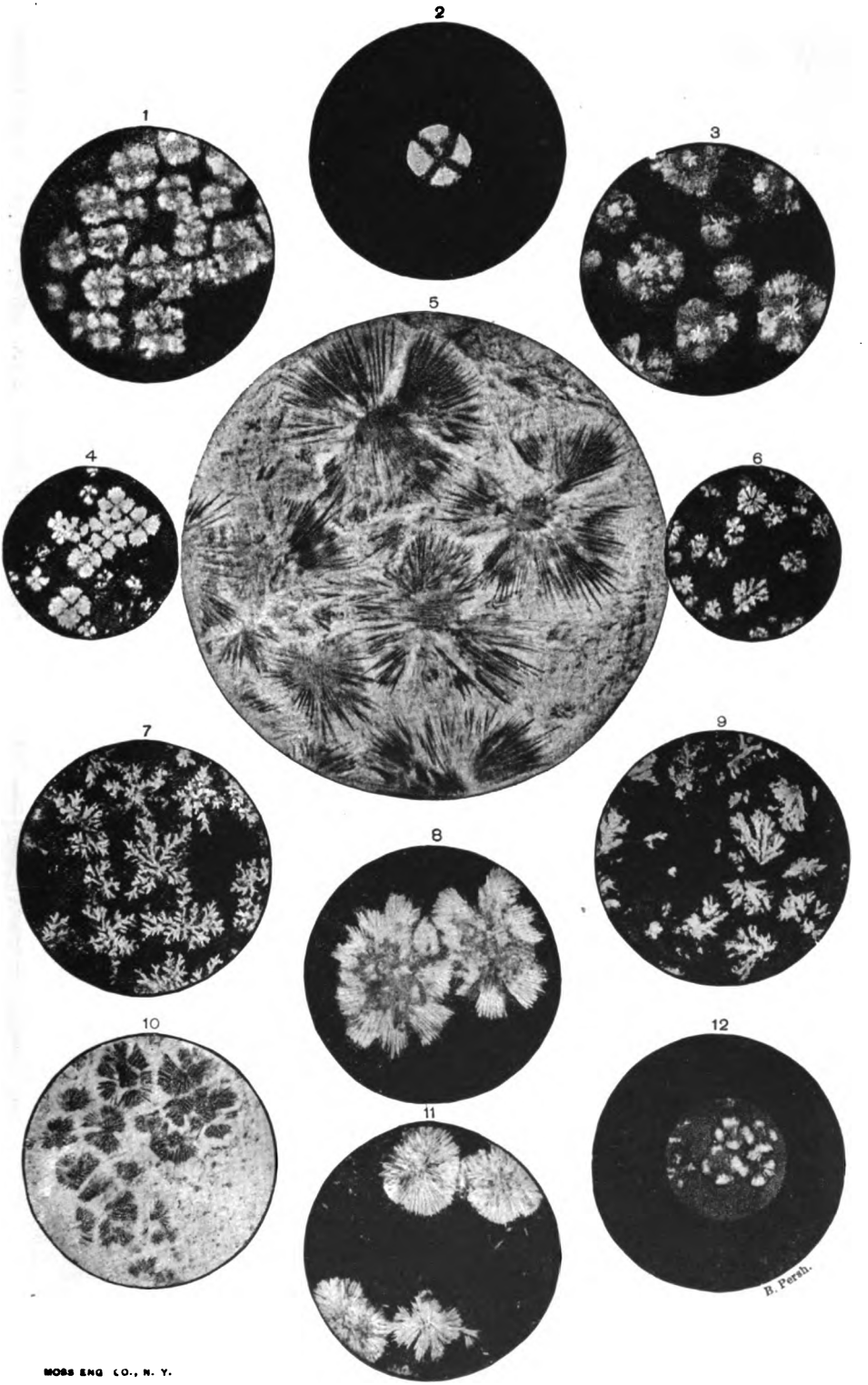

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PLATE VI.

CRYSTALLINE FORMATIONS OF VARIOUS FATS.

FIGS. 1 and 3. Respectively, primary and secondary crystals of loon fat. ×110.

FIGS. 2 and 8. Primary and secondary crystals of musk-rat fat. The primary (No. 2) are always very small, measuring about three one-thousandths of an inch in diameter.

FIG. 4. Crystals of oleo. ×140 diameters. (Extract of beef-fat.)

FIG. 5. Crystals of common lard by plain light. ×400.

FIG. 6. Secondary crystals of butter. ×110.

FIG. 7. Crystals of beef fat. ×140.

FIG. 9. Crystals of deer fat. ×140.

FIG. 10. Lard by plain light. ×140.

FIG. 11. Crystals of the solid fat of cottonseed oil. ×110.

FIG. 12. Neutral lard crystals, immature. ×140.

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REPORTS.

REMARKS ON THE FASOLDT TEST-PLATE.

By R. H. Ward, M. D., F. R. M. S., Troy, N. Y.

The committee to which was referred the plate of rulings presented by Mr. Charles Fasoldt has made progress, but is not yet ready to offer a formal report. An exhibition of the plate, however, by the maker himself according to his own ideas, is an incident of such scientific as well as historic interest, that it may be narrated now as an appropriate opening of the subject.

The plate consists of twenty-three bands ruled on a cover-glass, beginning at 5,000 lines to the inch and increasing by 5,000 each time to 30,000, and thence by 10,000 each time to or towards 200,000. The lines are ruled alternately longer and shorter, so that the 40,000 band becomes at each end a 20,000 band with interlying lines, and the "200,000" band should be seen, if resolved at all, as 100,000 band similarly interlined. The extraordinay mechanical skill of the maker and his success in ruling the lower bands, attach real interest to the plate and to his methods of studying it, in respect to the possibilities of fine ruling and of extreme resolution, an interest which is enhanced rather than diminished by the maker's lack of scientific education, his simple faith in the character and visibility of the highest bands, and his utter inability to apprehend the mechanical uncertainties and scientific absurdities involved in this belief. If he has done even a small portion of what he thinks, he has far surpassed all other experimenters and has earned and will receive much more credit than he claims.

Upon learning of the appointment of the committee, Mr. Fasoldt tendered a request that he might be allowed to be

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present "when the plate was examined," and kindly offered the use of his apparatus, and also his own services to "show the lines" to the committee at any time. Believing it to be of interest and importance to know exactly what he saw and how he saw it, I replied that while it would be impracticable for the committee as a whole to make the proposed arrangement, as a member of the committee I would gladly accept his offer to show the lines, and that the lines desired to be seen were the highest bands, from "120,000" upwards. No objection was made to this statement.

At an appointed time, one afternoon, the microscope was placed in a wooden cabinet which nearly excluded daylight, and light from a kerosene lamp with a large flat wick, placed edgewise at a distance of about two feet, was admitted through an opening in the cabinet on a level with the nose-piece of the microscope. The stand was a large and heavy one made by Mr. Fasoldt himself, with about ten inches of tube-length, including the objective, and furnished with a Bausch and Lomb $\frac{1}{12}$ in. hom. imm. objective claiming 1.40 n. a., and a 1 in. "periscopic" ocular by the same makers. The illuminating rays were brought to a focus at the side of the nose-piece and about one-fourth of an inch from it by means of a "watchmaker's glass" of about two inches focus, mounted as a "bull's eye" condenser, the best effect being gained with an achromatic one made for the purpose. The divergent pencil was admitted to the tube, and then reflected downward through the objective, by means of a cover-glass internal illuminator, claimed and patented by Mr. Fasoldt as his own. The peculiarities of this illuminator, aside from the substitution of Fasoldt's spring nose-piece for the ordinary society screw for carrying the objective, were an adjustment for withdrawing at will the cover-glass reflector from the optical axis, and an ingenious combination of shutters at the side, by means of which light was admitted only through a narrow slit that was adjusted in both width and position. With this arrangement a variety of bright or

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dark field effects were obtained by slight changes in the position of the lamp and the adjustment of the slit. When the image of the illuminating flame was formed by the objective just at the edge of the field of view and slightly out of the plane of the object, a transparent effect was produced over a considerable portion of the field, presumably by internal reflection at the bottom of the dry-mounted cover-glass, on the lower surface of which the lines were ruled, and in the bright portion of the field the lines of the lower middle bands were very easily and distinctly seen.

Starting from any of the coarser bands, where there could be no question about the lines, the plate was moved across the field by means of the steady mechanical stage, and the lines of successive bands appeared with distinctness but increasing fineness up to the band claiming 110,000 to the inch, which was seen with perfect ease, and the alleged 120,000 which was seen clearly and repeatedly, though with difficulty, while in the higher bands no trace or simulation of lines was perceived. The same limit was reached in several separate trials by the writer, whose eyes, however, by reason of long over-use, should set no limit against the reasonable claims of others presuming to go further. Mr. Fasoldt did not seem to recognize the lines as far up in the series as this; but his son, Ernest C., who was depended upon for most of the manipulation, was positive that he saw the lines in the "130,000, band, and none beyond that. Any possible importance attached to his judgment at this interesting point must be be viewed in connection with the fact that on another occasion he was satisfied that he "resolved" a "200,000" band. No measurement of the spacing of the lines was made at that time, and none is ready to report now.

Mr. Fasoldt's faith in the integrity and visibility of the still higher bands, which faith it is scarcely necessary to say is not known to be shared by any scientific man, seems to depend wholly upon his belief in the infallibility of his care-

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fully concealed method of ruling them, upon his impression that he has seen the lines as high as "150,000," and upon the equally firm impression of two or three other persons that they have seen all up to and including "200,000." These persons however admit that the higher bands furnish only passing glimpses and cannot be kept in focus and examined at leisure or shown to other persons as can be done with ease up to the "110,000." Is it possible that after looking long and intently at the latter, the retinal impression may remain and be recognized by the observer while subsequently gazing at the unresolved higher bands?

It would be evidently improper to undertake to anticipate the action of the committee as a whole, by saying exactly what should be considered sufficient evidence to establish the reality of certain of the lines and the fact of their resolution. But it will be noticed that the projecting alternate lines must greatly aid in the test of counting a measured portion of a band either with the micrometer or by aid of photography. It can scarcely be long impossible to make a satisfactory count of the band claiming to be spaced at "120,000," if it is correctly ruled, since the lines really to be counted are only at 60,000. And if, which is not incredible though not yet proved, this band should prove to be successfully ruled and to be resolvable by existing lenses, a fact that has been plausibly claimed but never yet really proved of any band of equal finess, then the study of the next two bands would be one of the most interesting problems in practical optics of the present day. At the same time it seems not improbable that photography may not only give us an easy count of lines plainly visible but extremely difficult to count otherwise, but may sometime show also the details of other bands that are permanently beyond the reach of direct microscopical vision.

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REPORT UPON THE COLLECTION OF SLIDES.

By D. S. Kellicott, Ph. D.

The following is a revised list of the slides belonging to the Society. A number of those named in former lists have become worthless and have been dropped. These are mostly such as were mounted in fluid.

Since the preceding report, Mr. Chas. E. Alling has contributed a number of excellent vegetable preparations, for the most part sections stained and mounted in balsam. Dr. Jas. E. Reeves has also contributed examples of his skillful work. Two slides of ruling, heretofore in the possession of the Committee on Micrometry, are now placed in the coltection.

The rules governing the custody of and circulation of these objects may be found at page 246 of the proceedings of the Cleveland meeting.

List of Slides.

Acer rubrum (23). Tranverse section. (Carmine).
Ærides odorata (21). Tranverse section of root. (Green).
Aristolochia sipho (24). Tranverse section. (Aniline green).
Ash (36). Tranverse section. (Picro-carmine).
Bacillus tuberculosis (38).
Bignonia capreolata (22). Tranverse section. (Picro-carmine).
Black gum (*Nyssa multiflora*) (17). Tranverse section. (Hæmatoxylin).
Capsella bursa-pastoris (1). Ovaries in various stages of development.
Cereus grandiflora (33). Tranverse section. (Picro-carmine).
" " (2). Tranverse section of ovary.
" " (3). Ovules with stigma and pollen-tubes.
Chester's Method of Dry Mounting (27–29).
Cristatella ophidioidea (26). Statoblast.
Crustacea (30–32).
Cuphea (5). Longitudinal and tranverse sections of the ovary.

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Cypripedium longifolium (34). Tranverse section of flower-stem.

Dry Mounts (27–29). Illustrating A. H. Chester's method with removable covers. (Proceedings Chicago Meeting, p. 143).

Dutchman's pipe (24). Tranverse section. (Aniline green).

Epithelium of lips (39).

Fasoldt's Micrometer Ruling (46). Compared by Professor T. C. Mendenhall.

Fasoldt's Micrometer Ruling, No. 261 (47).

" Test Plates (48). 5,000 to "200,000" lines to the inch.

Fraxinus sp. (36). Tranverse section. (Picro-carmine).

Gold (27). Crystals, fern-leaf form.

" (28). Crystals, prismatic.

" (29). Native and Petzite.

Kidney, tip of tongue, etc., of fœtus (41).

Larix Americana (35). Tranverse section. (Double stained.)

Leptadora hyalina (30, 31 male, 32 female).

Magnolia grandiflora (25). Tranverse section. (Double stained).

Maple (23). Tranverse section. (Double stained).

Micrometers (46–48).

Mistletoe (37). Tranverse section of leaf. (Double stained).

Monotropa uniflora (6). Section of ovary.

Nailer's consumption (42, 43).

Nyssa multiflora (17). Tranverse section. (Hæmatoxylin).

Oak (16). Tranverse section of stem.

Oenothera biennis (7). Section of ovary.

" " (8). Stigma and pollen-tubes.

Orchid (*Arides odorata* (21). Tranverse section of root. (Green).

" (*Cypripedium longifolium* (34). Section flower-stem.

" (*Peristera elata*) (18). Section of leaf-stem.

Papaver somniferum (9). Tranverse section of ovary.

Peristera elata (18). Section of leaf-stem.

Phoradendron flavescens (37). Tranverse section of leaf. (Double stained).

Pollen-tubes (1–15). Sections of various ovaries illustrating Mr. Kruttschnitt's views. (See Proceedings Rochester Meeting, p. 93, and Cleveland Meeting, p. 63).

Polyzoa (26).

Portulacca oleracea (10). Section of ovary,

" " (11). Section of stigma with pollen-tubes.

Pyrola elliptica (12). Ovules.

Renal tube-casts (44).

Statoblast of Polyzoa (26).

Sumach (19). Tranverse section of stem. (Aniline green).

Tamarack (*Larix Americana*) (35). Tranverse section. (Double stained).

Trichina spiralis (45). In human muscle, encysted and free.

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Trifolium pratense (13). Ovary, style and stigma.
Tube rose (20). Tranverse section of flower-stem.
Zinnia (14). Flowers with ovary.

REPORT OF TREASURER AND CUSTODIAN.

By Geo. E. Fell, M. D.

Pittsburgh, Pa., Sept. 2, 1887.

RECEIPTS.

August 13th, 1886, balance cash in hand	$517.85	
Receipts from sale of Proceedings	108.75	
Check from Chautauqua Association to pay expenses American Society of Microscopists	103.35	
Admission fees, dues, etc	306.00	
Total Receipts		$1,035.95

DISBURSEMENTS.

August 13th, 1886, to August 30th, 1887, Postage, circulars, expressage, etc	$200.57	
Proceedings	648.85	
Bill of Prof. F. A. P. Barnard for Centimeter Scale "A"	43.00	
Total Disbursements		$892.42
August 30, 1887, balance cash on hand		$143.53

Pittsburgh, Sept. 1, 1887.

The foregoing report has been examined and compared with the Treasurer's Account Books, and found correct. Vouchers for the expenditures, properly executed, are on file. Membership fees of the present meeting are not included.

C. M. VORCE,
JAMES E. REEVES,
Committee.

Schedule of Property in the Hands of the Custodian.

August 30th, 1887.

Volumes 1 and 2. Buffalo and Indianapolis Proceedings	112
Volume 3. Detroit Proceedings	72
" 4. Columbus Proceedings, 1881	78

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Volume 5.	Elmira Proceedings, 1882	81
" 6.	Chicago Proceedings, 1883	66
" 7.	Rochester Proceedings, 1884	54
" 8.	Cleveland Proceedings, 1885	88
" 9.	Chautauqua Proceedings, 1886	85

Bound Volumes of Proceedings, including Vols. 1 to 5	2
Bound Volumes of Proceedings, including Vols. 1 to 9	10

PAMPHLETS.

Micrometry	381
Memorial Charles Spencer	36
A Contribution to the Life History of the Diatomaceæ	26

In addition to this, all the property mentioned in report to the Elmira meeting, see page 287 Proceedings 1882, excepting the publications of this Society therein enumerated. Also electrotypes and wood cuts of the volumes 1882 to 1886 inclusive.

Standard Centimeter Micrometer "A"

Two copies by Mr. Fasoldt, viz: F_2 and F_8.

In making this report to the Society with which I have been associated so long in an official capacity, I desire to offer a few suggestions which, from a practical knowledge of the duties of Treasurer and Custodian, may be of benefit to the Society in the future. The duties of the officers, owing to the increase of membership, are continually increasing, and the time demanded to properly perform them is lengthening in like proportion.

To lighten the labors of the Treasurer, the custodianship should be placed in other hands. Let us have a permanent Custodian of the property of the Society to serve without pay, the office to be surrounded with any safeguards which may be requisite to the security of the property of the Society.

Regarding the Treasurer's duties, no one should be asked to perform the services of this office without at least being compensated for his expenses in attending the meetings, and

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as soon as the treasury will warrant it, he should be paid a sum that may, in a manner, compensate him for the time devoted to the Society's interests. Having now given to the American Society of Microscopists nine years of continuous work in an official capacity, as Treasurer and Custodian, requiring my attendance upon all of the meetings of the Society, I would wish for my successor in office the kind and considerate treatment accorded to me at all times by its membership.

Report of Treasurer on Spencer and Tolles Fund.

The subscriptions to this fund are as follows:

August, 1885,	The Royal Microscopical Society.....	$25.20
"	D. S. Kellicott	5.00
"	J. D. Cox.....	5.00
"	George E. Fell	5.00
"	John Kruttschnitt.....	5.00
"	F. S. Newcomer	5.00
"	E. H. Griffith	5.00
"	Dr. Charles Shepard	5.00
July, 1886,	Dr. R. H. Ward	25.00
August, 1886,	Chas. S. Fellows	10.00
August, 1886,	Total subscription previous to the Pittsburg meeting	$95.20

September 1, 1887.

The foregoing report examined and found correct. The amount is on deposit in the National Savings Bank of Buffalo to the credit of said fund.

C. M. VORCE,
JAMES E. REEVES,
Committee.

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Necrology.

MEMOIR OF ALLEN Y. MOORE, M. D.

By C. M. Vorce, F. R. M. S.

Allen Y. Moore, one of the youngest and best known members of this society, was born at San Diego, California, on the 12th day July, 1860. His father, Col. Orlando H. Moore, now on the army retired list, was then a lieutenant in the Sixth U. S. Infantry, and stationed at San Diego, his quarters being in the old town at the head of the beautiful day of San Diego, and about four miles from the present town of that name. There is an old adobe house of Spanish construction, with walls three feet thick and rooms only high enough for an ordinary man to stand upright within them, the subject of this memoir was born into a life destined to be fitful, brief and brilliant beyond that of most men of his time. The first seventeen years of his life were passed in roving and unsettled wandering, and were destitute not only of almost all the advantages, but nearly all of the comforts of the life of the ordinary American boy of that period. His opportunities were far less than those of the farmers's boy or the mechanic's son, yet, when cut down by the inexorable hand of death at the early age of twenty seven years, his was a well-known name in scientific literature, and his abilities were recognized by a large proportion of the learned men of the two hemispheres.

That one whose early years were deprived of those advantages of schooling and association with minds of trained intellectuallity which are ordinarily deemed essential to rapid mental development and thorough accomplishment in any branch of learning, should, in so few years and so early

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life, have placed himself among the leading minds in any specialty or direction of thought, is so far unusual that it seems worth our while to look into the circumstances which surrounded the youth and life of this brilliant young man, and endeavor to trace out the causes and influences to which he owed so phenomenal a success.

As before said, his early life was an unsettled one. About one year after Allen's birth his father was ordered to the east in consequence of the breaking out of the civil war. The family moved from point to point in the South until 1868, when Col. Moore was ordered to the Indian territory, where he was stationed at various posts until the spring of 1873, when he was ordered to Louisville, Kentucky, whither he brought his family, and here, for the first time, the boy Allen, now thirteen years old, had the opportunity of attending a regular school and mingling with other children. Up to this time and to some extent afterwards, his tuition was received from private tutors, to the best advantage that Col. Moore was able to obtain in the isolated places where much of the time was passed. Only a year was spent in Louisville, however, and in May, 1874, Col Moore was ordered to Fort Buford, Dakota. At this and other frontier posts the roving life was continued for about a year more, when Mrs. Moore was compelled by failing health to seek a milder climate, and settled with her children in Coldwater, Michigan.

In the Centennial summer of 1876 the family visited Philadelphia and other places, but Mrs. Moore's health still failing, she removed with her children to California in June, 1877, and settled in Tulare, where she died August, 1878. After his mother's death, Allen returned to Coldwater, where he remained, enjoying such school facilities as were there afforded, for a little over a year. In the fall of 1879 he entered the Cleveland Homœopathic Hospital College, where he graduated with credit in the spring of 1881, receiving the degree of Doctor of Medicine.

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While living in Tulare he began the correspondence and exchange of slides which ultimately made him so well and widely known to many who never personally met him. I have in my cabinet a slide received from him in August, 1877, accompanied by an request for exchange; the mount appearing to have been made in February of that year. The object is the flea of the cat, very well prepared and very neatly mounted between two covers cemented over a central hole in a wooden slip. This was the beginning of a correspondence which was continued at intervals until the Detroit meeting of the society in 1880, where I was approached by a boyish looking stranger, who called me by name and introduced himself as "your boy correspondent, Allen Y. Moore." I was greatly surprised at his youth, for although he was no mere boy, I had formed from correspondence with him and from his writings in the microscopical journals, the idea that he was a much older person. This was the beginning of a friendship between us which was never interrupted nor marred by a disagreement other than in matters of opinion.

It may well be supposed that the study of microscopy, which formed part of the course of study at the college, was peculiarly attractive to Allen and received his enthusiastic attention, and as it was pursued under the teachings Prof. J. Edwards Smith, who then held the chair of Microscopy and Histology in that institution, we can readily understand how the combined ability of teacher and pupil resulted in Dr. Moore's very rapid advancement and thorough mastery of the art, in which he was indeed no novice when he entered college. Almost immediately on graduating, Dr. Moore was given the chair of Microscopy and Histology in the college, on the resignation of Prof. Smith, and this position he held during the remainder of his life, and fulfilled its duties with eminent ability and success.

The meeting of this society at Detroit, at which time Dr. Moore became a member, was an important epoch in his

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life. There for the first time he met upon somewhat equal terms the opticians and skilled microscopists, from whom he quickly acquired a fund of knowledge that he added to unceasingly from that day foward.

I believe Dr. Moore attended every succeeding meeting of the society and was always to be found where the best optical work of microscopy was displayed. He had studied the optics of the microscope exhaustively in all aspects, and was in fact a thorough theoretical optician, familiar with all the causes and effects which enter into the result of the use of the best modern objectives, whether they related to the construction of the objective itself, the conditions under which it is used, or the nature and mounting of the object. He studied the correction of objectives not from the side of the effect but from the side of the cause, hence he never moved the adjusting collar without knowing not only exactly why he would move it, but in what direction it should be moved, and what the result would be. The nature and effect of immersion media, and of mounting media, were studied by him in the same manner, and the methods of illumination with equal thoroughness. In this direction were pointed his studies in the silver plating of rulings and diatoms. He also made many experiments in the composition of mounting media of high refractive index, experimenting particularly with various metalloids, he produced independently and at about the same time as Prof. H. L. Smith, the sulphide of arsenic medium of 2.48 index, and had prepared a medium less highly colored which he believed would prove valuable, but the formula for which has not been preserved.

It was while living at Coldwater, in the year 1876, that Dr. Moore made his first appearance in the field of Microscopical literature, in a communication to the *American Journal of Microscopy*, on the preparation of silver crystals, which appears in the March number of that year. At the time of writing that article he was not yet sixteen years of age,

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but I feel certain that no person to whom the author was unknown would, on reading that article, believe that it was written by so young a person. From that time forward he was a frequent contributor to the pages of the microscopical journals published in this country. A certain spice of fun in his disposition caused him to publish some of his articles over the *non de plume* of "Nelly A. Romeo," an anagram of the letters of his name.

After being seated in the chair of Microscopy and Histology at the college, Dr. Moore determined to pursue the work of Microscopical Analysis as a business, instead of entering into general practice. For some time before he graduated he had been engaged in Microscopical Analysis for physicians in active practice, devoting especial attention to urinary analysis, tumors, etc. In this work he was rapidly gaining prominence, and at the time of his death had already acquired a patronage extending throughout Ohio and into the adjoining states. Personally, I have little doubt that had Dr. Moore been spared a few years longer, and especially had he been less modest and unassuming and possessed more of that faculty which even physicians are occasionally blessed with, known as blowing his own horn, he would have earned a national reputation as an expert in microscopical analysis and diagnosis, especially in Histology and Pathology.

Dr. Moore was married at Coldwater, in December, 1885, to Miss Gertrude Neal, of that place, who evinced a more than wifely interest in his professional work and soon became so expert that she aided him largely in his work, both in analysis and in the preparation of slides. Not a few of the slides sent out by Dr. Moore were the work of her fair hands.

The skill which Dr. Moore possessed in all manipulative processes is too well known to the members of this society to require mention, his slides exhibiting simple and multiple electric arcs, microscopical geissler tubes, silver-plated and

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gold-plated diatoms and rulings, double stained blood discs, stained diatoms, crystals of metals, tube casts, and various other objects, such as whole insects, etc., are familiar to most microscopists. In the optics of the microscope Dr. Moore took the greatest interest. He was making great use of the micro-spectroscope, and had become expert in its use. He had tested for himself the Wenham principle of adding a supernumerary front lens to an objective, so as to increase both the power and angular aperture of the lens, and had thus converted a $\frac{1}{2}$ into a $\frac{1}{6}$ objective. His last work in this line was the elaboration of the formula for a concave lens of excessively minute radius, which was to be applied to the front of an objective and filled with a fluid of a refractive index which should, while harmonizing with the corrections of the objective, yet materially increase its aperture, and consequently its power of resolution. He had given an order for the construction of one of these minute lenses to Messrs. H. R. Spencer & Co., but I believe the work had not been commenced at the time of his death. Some experiments made by Dr. Moore appeared to demonstrate that this method of increasing the aperture of objectives was entirely feasible where sufficient working distance was present in the first place.

Although he had inherited a constitution far from robust, which the privations and hardships of his youth had not served to strengthen, Dr. Moore, perhaps on account of his army training, had always an almost utter disregard of any care for his health, and seemed to fear no exposure to either heat, cold or wet. He never seemed to suffer from these exposures, but his apparent recklessness as to his health caused much uneasiness to his friends, who feared to see him a victim to consumption. This was not to be, but an equally fatal, though unlooked for, result was in store. On the 8th day of April, 1887, he was suddenly taken ill, and although he would not at first believe that his illness required the attendance of a physician, it speedily became ap-

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parent that he was seriously ailing and medical aid was summoned; but despite all that the best medical skill could do he rapidly sank, and on the 16th day of April he breathed his last, his painful illness having lasted just one week.

With all his abilities and acquirements, of which he could not himself be ignorant, Dr. Moore was the personification of modesty. He was so thorough in all matters that he was always sure of the position he took upon any question, and expressed his views clearly and boldly when they were challenged, but he never obtruded his own opinion unsought or officiously, and I do not believe a boastful or conceited expression was ever heard from his lips. His loss is one long to be mourned and his place in the front rank of microscopists will not soon be filled.

MYRON C. DAVIS.

Born in Rutland, Massachusetts, March 4th, 1847; died January 18th, 1887, at Santiago, Cuba. Mr. Davis was member of the Iron City Microscopical Society, and became a member of the Chautauqua meeting of the American Society of Microscopists. He was a machinist by trade, but, since 1876, gave his chief attention to the mining of manganese ore in which he became an enthusiastic and accomplished master. At the time of his death, it is probable that there was not a locality in the country in which manganese had been found or was supposed to exist with which he was not conversant. He had gone from his home in Pittsburgh, Pennsylvania, to Cuba to examine, with a view to opening some manganese mines and was stricken with yellow fever. Though having little time for microscopy, he took an active interest in all matters connected with the local society, and had his life been spared would have proved himself an efficient and useful member of the national organization.

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PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MICROSCOPISTS

MINUTES OF THE TENTH ANNUAL MEETING.

The Tenth Annual Meeting was held in Pittsburgh, Penn., August 30-31, September 1 and 2, 1887.

The Society was called to order at 10 A. M. August 30, by the President, William A. Rogers, in the Chapel of the First Presbyterian Church. The President spoke as follows:

"The American Society of Microscopists convenes in the City of Pittsburgh, upon the invitation of the Iron City Microscopical Society, under auspices which give promise of a most successful meeting. Already the evidences are seen on every hand The provision which has been made for the successful prosecution of our work, the cordial greeting upon the arrival of every incoming train, the enterprise shown by our faithful ally, the Press, — all these things convince us that we have been fortunate, indeed, in the selection of Pittsburgh as our place of meeting.

"It will give us pleasure to listen to the more formal expression of the welcome which has been extended to us, not merely because we appreciate good fellowship, but because we desire to make a permanent record of this expression in our Annual Proceedings. The members of this Society hardly need an introduction to the President of the Iron City Microscopical Society, a society which has given us such a splendid example of vigorous activity. We shall be glad to hear from Mr. Mellor."

Remarks were then made by Mr. Charles C. Mellor, President of the Iron City Microscopical Society. He spoke appropriately as follows:

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"It is but five years since a few of us met and organized the Iron City Microscopical Society, and I think I can safely say that none of us then supposed that it would in their time attain its present size and importance; and I am quite sure that not a single one of us even dreamed that we would have the courage, or receive sufficient encouragement from our citizens, to justify us in inviting the American Society of Microscopists to hold its Tenth Annual Meeting in our city. But these things have both come to pass,— our local society has attained such a size and importance that it does not need a microscope to discover it, and we hope it is now one of the permanent institutions of our city, and the American Society of Microscopists is here.

"This is a proud day for our local society, and I feel confident that your presence here — the sight of so many persons interested in the beautiful and useful science of Microscopy — will stimulate all our members to greater efforts to place our society on a firmer footing and to do better work and more of it.

"To the citizens of Pittsburgh, who have so kindly helped us with encouraging words and also substantial assistance, I desire in the name of the Iron City Microscopical Society to tender our warmest thanks, and also to the Press for notices published. The address of welcome will now be given by the Chairman of the Reception Committee. I have the honor to introduce to you Dr. B. C. Jillson."

The address:

"*Mr. President and Members of the American Society of Microscopists:*

"You have honored Pittsburgh by selecting it as the place of your annual meeting. We appreciate the honor. We most cordially welcome you to our city. We will do all we can to make your stay profitable and pleasant.

"Pittsburgh is known far and wide for its energy and industry, especially in the line of metallurgy and manufactures. Like other large and busy cities, there are few here who are specially interested in that particular branch of science to which you are devoted. A few years ago less than half a dozen persons owning microscopes occasionally met in an informal manner for study and consultation. That number has increased more than ten-fold, and is now a regular organization with rooms for study and work. We are young. We have as yet done but little. We hope to do more in the future, and we feel sure that your presence here will encourage and stimulate us to greater exertions. There is a goodly number of persons here who appreciate abstract science, and are willing to spend both time and money for its advancement. On yonder hill are two institutions which have carried the names of Pittsburgh and Allegheny to every part of the scientific world. The Allegheny Astronomical Observatory, by the genius of its principal observer, has, as you well know, added much to the world's knowledge

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of the physics of the sun, the character of our atmosphere, and the invisible part of the solar spectrum; while in the other building, in a way peculiarly his own, the gifted director is able to grind lenses, polish prisms, rule spectroscopic gratings, and make other instruments of precision, which are said by scientific experts to be unequaled. This much has smoky, busy Pittsburgh done for abstract science; a contribution of which we are by no means ashamed.

"Your Society has an enviable reputation for industry. We know that you came here to work and that your time is limited. We are also well aware that man cannot live by the microscope alone, and that most of you are engaged in some other business, profession or pursuit. In a city so large as ours with so many and varied points of interest, it would be surprising if there was not something in the particular line of each well worth your attention. If you are a metallurgist, our numerous furnaces are open to you. If physics or mechanics is your specialty, there is much to be seen in our many and varied forms of manufactures. Should you desire to investigate the applicature of electricity in new and varied forms, we can show it you. Here chemistry has many applications in the arts. Some of our bridges embrace appliances so unique that students from Ann Arbor and Hoboken have considered them worthy a visit. Our Geology has some features of interest. Directly across the river from your headquarters a section of coal is well exposed, showing at a glance the peculiarities of the Pittsburgh coal seam, which extends over an area of 20,000 square miles, and has been made the base line of Carboniferous Geology. There are, right here in our city, extensive river terraces of the Champlain era, two hundred feet above the present river beds. Thirty years ago oil was discovered in the rocks of Western Pennsylvania, and our city became the head-centre for oil refiners, the invention and manufacture of drilling tools, oil tanks, etc. It is hardly three years since one of our enterprising citizens, living in the very heart of the city, prompted by the laudable desire to know what was in the rocks beneath him, erected a derrick in his yard. He struck — not oil, but gas, and a new era dawned on our city. Pittsburgh has been called 'the smokiest city on the face of the earth.' It is fast becoming the clearest and cleanest city in the Union. In this valley, containing more than three hundred thousand people, natural gas is universally used for heating and domestic purposes; while its application to our manufactures has in every case improved, and in many completely revolutionized, these branches of industry. This vicinity, as you well know, is rich in historical associations. The land on which our city stands passed from the Indians to the French, from the French to the English, and from the English to our own government. At 'The Point,' where the Allegheny and Monongahela unite to form the Ohio, Fort Duquesne and afterwards Fort Pitt were erected. Both these forts have now disappeared, but a block house, once a part of Fort Pitt, still remains, an

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object of interest to visitors. On the hill where our court house now stands General Grant and his Highlanders were massacred while on their way to Fort Duquesne. Ten miles to the east of us is the battlefield where Braddock was defeated. We have planned an excursion for to-morrow to some large steel works built on this historic spot. But I have said enough by way of introduction to an invitation I am about to give. If any of you wish to visit any points of interest in or about our city, if you will make known your desire to any member of the Reception Committee we will take great pleasure in furnishing you with a passport and guide to the locality.

"In behalf of the City of Pittsburgh in general and the Iron City Microscopical Society in particular, it is my pleasant duty to again cordially welcome you to our city, and to express the hope that your session may be both profitable and interesting."

The President replied in these words:

"I am sure that I express the sentiments of every member of this Society in thanking you most heartily, as the representative of the Iron City Microscopical Society, for the cordial welcome extended to us. The local committees of your Society have done their work faithfully and well. It remains for us to justify this expenditure of time and money, this careful preparation, this thoughtful attention, by a session which shall mark an era in our history.

"I suspect that few members of this Society realize the amount of what is sometimes called 'dead work' which needs to be done when a local society undertakes to see that nothing shall be left undone to make a meeting of this character an unqualified success. 'Dead work' in science is that careful, previous preparation which attracts no attention, but which, nevertheless, must precede every brilliant discovery; it is the arming, the equipping and the drilling of an army in preparation for one short hour of battle. The citizens of a large city, especially of a city which has such a varied range of industries as the City of Pittsburgh, have many interests in common with us. We ought to be mutually helpful to each other. There is hardly a problem connected with the practical industries of this city in which the microscope may not be made a useful auxiliary. I am not now speaking of technical scientific investigations, but rather of the aid which it may give in the practice of the useful economies of daily life. If the agricultural chemist can make ten spears of grass grow where only one grew before, he who uses the microscope as a means to an end ought to be able to find many ways, not only of adding to the knowledge and the happiness of the world, but of increasing its commercial prosperity as well; for I hold that it is not an unworthy object to show how one dollar may be made to do the work of ten.

"The claims of the microscope as a useful ally in almost every pur-

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suit in life are coming to be more generally recognized, and this Society has a well-defined obligation to extend the field of its applications. While the American Society of Microscopists must ultimately stand or fall according to the scientific character of its published proceedings, and while we must set a high ideal before us in this respect, it has other and equally well defined obligations to which we ought to give due attention. Pure science is a necessity in this world, but it is not the only thing worth living for. To add to the sum of useful happiness of mankind is a worthy object. I suspect that a community composed entirely of technical specialists in science would not prove to be an ideal one in every respect. In fact, if such a community should be able to struggle through the first stages of a doubtful existence, I doubt if its attractions would be such as to secure a large accession to its numbers. We live in a busy world of activities, in which every interest ought to contribute something to every other interest.

"Since one of the chief objects of your Society is the critical study of the useful problems of life, we ought to extend a hearty welcome to every honest worker. I appeal especially to young men to enter our ranks; to young men who are just entering upon professional life; to young men who have just completed their college course; to young men who are still in college; to lovers of nature everywhere who are sincere worshippers at her shrine. As far as I have any influence it shall be given in favor of aid and encouragement to every sincere effort to seek the truth. The mere theorist has no place among us. The scientific crank we abominate and will not tolerate. The honest student of nature shall always find a home among us and a hearty welcome at all times.

"Members of the American Society of Microscopists, I congratulate you on the prospect of a successful meeting. We are now ready to enter upon the work before us."

Prayer was then offered by Rev. John Fox.

The Secretary then reported for action the recommendations of the Executive Committee.

1. That By-law seven which reads as follows be rescinded: "The Executive Committee shall not act upon a recommendation to membership until the iniation fee, $3.00, and one year's dues, $2.00, shall have been received by the Treasurer."

On motion the recommendation was adopted.

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2. The following were then elected members of the Society: William W. Wishard, Indianapolis, Ind.; Herbert M. Hill, Watertown, N. Y.; George M. Sternberg, Baltimore, Md.; Freda Detmers, Columbus, O.; Morgan W. Ayres, Upper Mont Clair, N. J.; William C. Gardner, Mont Clair, N. J.; Arthur R. Warren, North Madison, O.; Mark Francis, Glendower, O.; Charles W. Brown, New York City; Robert Robotham, Syracuse, N. Y.; George S. Allen, Mont Clair, N. J.; Geo. W. Rafter, Tuscaloosa, Ala.; Veranus Alva Moore, Washington, D. C.; Ira W. Lewis, Dixson, Ill.; J. D. Griffith, Kansas City, Mo.; James Eichberg, Cincinnati, O.; Henry Dickson Bruns, New Orleans, La.; P. H. Dudley, New York City.; William H. Sheib, Wheeling, W. Va.; L. D. Wilson, Wheeling, W. Va.; F. W. Entrikin, Findlay, O.; Henry B. Ward, Troy, N. Y.; Lewis M. Early, Columbus, O.; A. J. Hoenny, St. Louis, Mo.; Hiram De Puy, Pittsburgh, Pa.; C. G. Jennings, Detroit, Mich.; B. C. Jillson, Pittsburgh, Pa., and J. M. Stedman, Ithaca, N. Y.

3. That the following be made a By-law of the Society: All motions or resolutions relating to the business of the Society shall be referred for consideration to the Executive Committee before discussion and final action by the Society.

After discussion the rule was adopted by a unanimous vote.

4. Special attention was called to the Spencer-Tolles Memorial Fund, and the hope expressed that it may be materially increased during the present meeting. The President urged the increase of the fund so that the annual income shall be a sum large enough to make it worth while to appropriate it for the purpose of encouraging original research, especially by our young men.

Notice of deceased members was then read by the Secretary. Five members have died during the year: Myron

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C. Davis, Bernard Persh, Allen Y. Moore and Chas. Blasdale. [For Necrology, see page 329].

C. M. Vorce then read a carefully prepared Memoir of Dr. Allen Y. Moore. [See page 329].

R. H. Ward described an erecting apparatus designed especially for use with the binocular microscope.

Dr. R. H. Ward exhibited an erecting arrangement especially intended for use with the Wenham binocular, but also available for a monocular not adapted to receive an erecting objective in the draw-tube. The apparatus, combined with any convenient camera lucida, has also proved useful as an "embryograph" for drawing or measuring large objects at any desired amplification, from natural size (or even less) up to 20 or 30 diameters or more, as in making or copying diagrams, etc. For the lowest powers it may be combined with an ordinary tank microscope, or even with a compound body supported on a bull's-eye pillar, or on a chemical retort stand, by means of its various sliding rings.

For the objective a compact form, as a "solid triplet," is used, a 2-inch, giving a suitable range of powers and good optical effect, but a 1-inch, taking less room, and being therefore preferable for small stands. This lens is mounted at the top instead of the bottom of its brass tube, so as to stand close below the binocular prism, while the bottom of its tube has an inside society-screw for carrying in the usual position, only a little lower, another objective which serves as an erector. The lower lens acts like a telescope objective or an achromatic condenser, forming in the focal plane of the upper a reduced and inverted image of the object at its own distant, and in this case, lower, conjugate focus, which image the upper lens, the acting objective, magnifies and erects. The adapter connecting the two has an adjustment for length, by screw or slip-tube; and the lenses are worked at their adjacent sides at so near their principal focal distances that a half-inch change of distance, together with two or three inches change of draw-tube, gives a range of powers of from 5 to 50 diameters, or a lower range by a different selection of lenses, with an inversely corresponding width of field.

The illumination for the lowest powers is easy for opaque objects, but for transparent effects it may require some form of white-cloud contrivance, of which a white card laid upon the mirror, with its corners turned down to hold it in position, and intensely lighted, is always available and often efficient. The definition is, of course, somewhat impaired, — few persons would expect to improve it by an erector of any sort — but is sufficiently good for dissecting minute objects, picking out fibres, diatoms, etc., or drawing diagrams. The increased focussing distance

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of this arrangement prevents its adaptation to very small stands, and brings the ocular to a somewhat higher level than usual. This increased height, which is of real advantage in drawing diagrams or large plates, is not inconvenient when the microscope is in an inclined position, but requires one to stand, or use a low table or high stool, when it is erect. As the two-thirds objective is also an excellent lens for general use, the only special cost of the arrangement is that of the adapter and the inexpensive 1 or 2 in. lens.

Discussed by W. H. Seaman, Edward Pennock, and George E. Fell.

Mr. John A. Brashear extended an invitation to the Society to visit the Optical Works of Brashear & Co., and the Allegheny Observatory. The invitation was accepted and the thanks of the Society tendered for the courtesy.

Mr. C. C. Mellor announced that there would be excursions to places of interest after the adjournment of the afternoon session and that members of the committee on reception would escort the several parties.

Adjourned to meet at 2 P. M.

TUESDAY, August 30, 1887.

Society called to order at 2 P. M., President Rogers in the chair.

On proper recommendation, the following were elected members: William J. Prentice, Allegheny City, Pa.; J. H. McRoberts, Pittsburgh, Pa.; A. G. Kornblum, Pittsburgh, Pa.; William H. Keck, Allegheny City, Pa.; W. A. E. Drescher, New York City.

Papers were then read as follows:

1. *The Length in millimeters, and the Parts included in Microscopical Tube-length by the various Opticians of the World; also the Thickness of Cover-glass for which Unadjustable Objectives are corrected.*—Simon H. Gage.

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Discussed by W. H. Seaman, Carl F. Lomb and H. H. Turner.

2. *Some Experiences in Crystallization by Cold.*—Frank L. James.

In discussing this paper, W. H. Seaman said: "My experience in the line of work described by Dr. James shows its uncertain results. Out of four lots of Ethyl Gallate slides, only one was first class, some of which are in the hands of members here present. Temperature effects chemical action, as in the case of sodium chloride and zinc sulphate, that at a temperature of between 35°—45° F. undergo reciprocal change, but at higher temperatures the action reverses. The conditions effecting crystallization are principally the temperature and dryness of the air, and the density of the solution. Crystallization is the arrangement of the molecules of a substance along the lines of polar forces, which act slowly, hence, evaporation must be slow to admit of the solvent fluid remaining a sufficient time to secure the necessary mobility of the molecules. Sometimes too rapid evaporation may be prevented by adding a little gum water, first suggested by one of our number. Only by this means have I succeeded with quinate of quinine which otherwise dries up on the edges of the drop to an amorphous mass. Too strong a solution or too great a quantity in one place has a similar effect by forming an opaque body."

3. *Additional Notes on Certain Rotatoria.*—David S. Kellicott.

Discussed by S. H. Gage.

4. *Some New and Rare Infusoria.*—David S. Kellicott.

Read by title.

5. *Apochromatic Objectives.*—Ernst Gundlach.

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The paper was read by Henry H. Turner

6. *Observations on the Effects of Powerful Electric Currents on the Tissues of Animals.*—George E. Fell.

Discussed by S. H. Gage, W. J. Lewis, L. D. McIntosh and W. P. Manton.

Under the order informal announcements of new apparatus and methods, Dr. James E. Reeves explained his method of preparing and mounting sections of various animal tissues. Remarks on his methods were made by S. H. Gage, W. H. Seaman, W. P. Manton and others.

Adjourned to meet at 8 o'clock to listen to the President's annual address.

[After the close of the afternoon session, parties visited places of interest. Notably, a large party visited the Allegheny Observatory and the Optical Laboratory of John A. Brashear].

EVENING, August 30, 1887.

In the evening the President read the annual address to an appreciative audience, composed of members of the Society and citizens. Mr. C. C. Mellor presided and in introducing the President said: "In accordance with the custom of American Society of Microscopists, we are assembled this evening to listen to the annual address of its President. The name and the work of him whom it is the honor of the Society to have this year as its chief executive officer, are too well known to need any words or comment from me. for wherever microscopy or micrometry is known the name of William A. Rogers is equally well known. I have the honor of introducing to you Prof. William A. Rogers."

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The address was a profound discussion of an important question in physics, entitled, "The Microscope as a Factor in a study of the Behavior of Metals under Variations of Temperature."

WEDNESDAY, A. M., August 31, 1887.

President Rogers in the chair.

Action on recommendations of the Executive Committee:

1. Election of the following members: William James, Columbus, O.; Susanna S. Phelps Gage, Ithaca, N. Y.; Frank Vandepoel, Newark, N. J.; W. J. Riggs, Allegheny City, Pa.; E. A. Mundorff, Pittsburgh, Pa.; Frederick Gærtner, Pittsburgh, Pa.; Frederic N. Pease, Altoona, Pa.; Carl H. Van Klein, Dayton, O.

2. On the suggestion of a member, the Committee recommended that the election of the nominating committee, which usually occurs on the morning of the day before final adjournment, should this year take place at the close of this, Wednesday A. M. session. On motion the election of said committee was made the order at 12 o'clock.

3 The invitation of the Westinghouse Electrical Co., to visit their works was accepted and the thanks of the Society to the officers for the courtesy. The time for the visit was fixed at 4:30 P. M. Thursday.

Dr. H. Dickson Bruns offered the following, which under the rules was referred to the Executive Committee.

"*Resolved:* That hereafter the Society adopt as a rule of proceeding the admirable suggestion of Dr. James E. Reeves, that each afternoon session of the annual meeting be devoted to practical work in the preparation and display of microscopical objects."

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PAPERS AND DISCUSSIONS.

1. *Cements and Waxes.*— W. H. Seaman.

Professor Wm. H. Seaman said the matter he desired to present was a kind of summary of interesting results of the meetings of the Washington Microscopical Society.

Early last winter Dr. Thomas Taylor suggested that a varnish known as Berry's Hard Finish, might serve as a cement. This varnish is in very extensive use for coating wood in its natural colors, and hence easily obtained everywhere. Dr. C. T. Caldwell, upon trial, found the material not only useful as a cement, but also as a mounting substance proper. Since, a number of the members have used it with the most favorable results. Slides were shown in which insects had cleared up well without any previous preparation. If too thick the material may be thinned with turpentine. One of its advantages is that it does not precipitate when brought in contact with aqueous solutions to anything like the extent that balsam does.

Professor Seaman also described a new imbedding material for section cutting. He said: Dr. J. H. Blackburn, of the Government Insane Asylum, in attempting to carry out the Reeves, process of mounting failed entirely to get satisfactory results with what was sold to him by the local druggist as myrtal wax, which he desired to try at the suggestion of Dr. Miller. On returning the wax and stating there must be some other substance called myrtal wax, he received an article that gave perfect satisfaction, so much so, indeed, that he found it better than paraffin, and substituted it for that. Upon testing, the speaker found it was the Japan wax obtained from *Rhus succedanea*, now an extensive article of commerce. The substance is peculiar in its wide range between its fusing and solidifying points. It solidifies without wrinkles and sticks close to an imbedded object — qualities that render it especially valuable to the

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section cutter. It is not strictly a wax at all, but a fat, since it consists chiefly of palmitic acid, and is capable of saponification. Dr. Blackburn had shown whole brains saturated with it so perfectly, and preserved so naturally except color, that there seemed to be no reason they could not be employed as models for class demonstration. To all appearances they are permanent. The substance may be obtained from wholesale druggists.

R. N. Reynolds, of Detroit, remarked that Berry's Hard Finish is substantially Zanzibar copal dissolved in turpentine.

Discussion.—Dr. Jas. E. Reeves inquired at what temperature waxes melted.

Prof. Seaman replied that they liquify from 53° to 55° C., solidifying again at 40° C.

R. N. Reynolds recommended the use of "Berry Brothers' Finish." He said it is a gum obtained from Zanzibar; he saws a slice and grinds it to any desired thinness; benzol does not dissolve it; he possesses old mounts prepared with this material and considers them durable.

Dr. J. E. Reeves remarked that any wax with a melting point above 140° F. is useless for imbedding; a brief exposure to such a temperature would destroy the tissue; on cutting this wax Meynert finds it too brittle. He said he had had great trouble in getting proper wax and considered paraffin the best material to be had at present.

Prof. Seaman replied that he thought Japan wax would prove to be useful as its melting point was 120. F. and thus below what Dr. Reeves considers safe.

Dr. F. L. James said he had studied all cements in use and regarded white zinc, as prepared by a formula of his already published, as nearly perfect wherever a cell is required.

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His attention had been called, by Dr. Taylor, to the "hard finish" already mentioned; its translucency can be cleared by pure benzol; pure benzol will crystallize at the freezing point, the sort used by him was marked "crystalizer."

2. *Method of Estimating the Number of Trichinæ in Meat.*—Simon H. Gage.

Discussion: Edward Pennock asked if this method would not be erroneous inasmuch as the Trichinæ would be cut in two and thus doubly counted?

Dr. F. W. Entrikin inquired by what means the young Trichinæ were distributed.

Prof. Gage said the young were bred in the intestines, bore their way into the blood vessels, and are thus carried to their destination.

Dr. Entrikin said Dr. W. B. Rezeur had made many experiments to determine this fact and had concluded that the ova alone were thus carried along.

3. *Disease Germs—Another Illustration of the Fact that Bacteria cause Disease.*—Thomas J. Burrill.

Discussion postponed owing to lack of time.

4 *Ending and Relation of the Muscular Fibers in the Muscles of Minute Animals.*—Susanna S. Phelps Gage.

The President then announced that the hour had come for electing the Nominating Committee and called for free nominations for the committee. Nine members were nominated. Simon H. Gage and John J. B. Hatfield were appointed tellers and the ballot ordered.

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The Tellers reported the seven receiving the highest number of votes as follows: William J. Lewis, S. M. Mosgrove, George E, Fell, C. C. Mellor, George H. Clapp, Frank L. James and W. P. Manton.

The chair declared these gentlemen the Nominating Committee, to report a full list of nominations for officers Friday morning.

Adjourned.

WEDNESDAY, P. M. AND EVENING.

The Society as guests of the Iron City Microscopal Society, assembed at 2 P. M. on board the steamboat Mayflower, for an excursion twelve miles up the Monongahela to Braddock, the historical Braddock's Field. The beautiful scenery of this part of the Monongahela, the perfect air and sky, the very pleasant social reunion of the hour and the extreme thoughtfulness of the local Society, rendered the trip most enjoyable; especially so was the "working session" announced by President Mellor soon after the boat left the dock on the return. The hungry company showed unmistakable signs of appreciation.

The Mayflower landed the party at the dock of the Edgar Thompson Steel Works, thence they were conveyed by train to the works themselves, where all witnessed with delight the various processes in the manufacture of iron, the conversion of Bessemer steel, the rolling of steel rails, etc. The attendants of Phipps, Carnegie & Co., took great pains to explain the various operations and thus won the deserved hearty thanks of the Society.

In the evening there was a reception in the parlors of the Monongahela House. The social reunion was continued until a late hour.

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THURSDAY, September 1, A. M. Session.

President Rogers in the chair.

The following were elected members: Frederick A. Holton, Kansas City, Mo., T. W. Johnson, Danville, Ind.

On recommendation of the Executive Committee it was decided to hereafter deliver the proceedings at the expense of the members, *i. e.*, by special arrangements by express.

The Executive Committee recommended the following substitute for the resolution regarding working session by Dr. Bruns. [page 346]: That the Executive Committee be requested to secure if possible for each half day's session of the Society, at its next annual meeting, one paper upon some subject of microscopical technique with accompanying demonstrations. This is not to interfere in any way with the regular working session.

The whole matter was, by resolution, referred again to the Committee by the Society.

Regarding the matter of tube-length [See p. 168] the following was recommended and adopted: Whereas the question of tube-length entered as a prominent and important factor into the consideration of the subject of nomenclature and powers and was repeatedly discussed at length both in Committee and by the Society; and, whereas, the Society at the Rochester meeting deliberately and unanimously committed itself to a policy in regard to the item of tube-length, it therefore seems too early to formally re-open the subject; and it is recommeded that it lie over for the present.

The Committee recommended the following disposal of the Spencer-Tolles Fund: The income of the Spencer-

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Tolles Fund — the principal forever remaining intact — shall be applied to the establishment of a Fellowship for the encouragement of special research, and be held under the yearly appointment of the Society. The recipient of this Fellowship must be a member of the Society and must give evidence of special fitness for the appointment. The results of the research undertaken shall be communicated to the Society at the termination of his appointment. Adopted.

Subscriptions to the fund were then taken as follows: H. R. Spencer & Co. $25.00, W. A. Roger's $25.00, Iron City Microscopical Society, $25.00, S. H. Gage, $10.00 *The Microscope* $10.00. H. D. Kendall, $10.00, W. P. Manton, $10.00, T. J. Burrill $5.00, C. G. Milnor, $5.00.

The discussion of papers was then entered upon.

1. *Foot Rot in Sheep.*— Mark Francis.

The author being absent, Dr. H. J. Detmers read the paper.

2. *Exhibition and Description of a Microscope made about 1738.* — C. C. Mellor and Jacob Henrici.

3. *Some Easy Methods of Testing Photographic Lenses.*— H. H. Turner.

4. *The Comparative Size of Blood Corpuscles of Man and Domestic Animals, with twelve photomicrographs of blood.*— Freda Detmers, In the absence of the author, Dr. J. H. Detmers read the paper.

In discussing it Mr. Vorce said, "The better way in measuring blood corpuscles, is to test as large a number as possible, rather than to measure the largest and smallest, as no two series agree.

5. *On a Microscopical Slide Catalogue.*—R. H. Ward.

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Dr. Ward presented a copy of this record to the Society.

6, *The Tape Worm—Methods of preparation for Museum and the Microscope.*—J. M. Stedman. Read by Mr. E. H. Sargent. Mounted specimens of much beauty were presented in illustrating the paper.

Prof. S. H. Gage remarked that on looking up methods of preparing these forms, he found only the one by drying the segments of the worm on paper. He wished to compliment Mr. Stedman on his success in preparing these elegant specimens.

7. *A Description of Ergasilus Chautauquaensis, and a list of other Entomostraca found at Chautauqua Lake, in August 1886.*—Charles S. Fellows.

The paper was read by Dr. Lester Curtis, and illustrated by a series of well mounted specimens.

8. *Are Lupus and Tuberculosis Identical.*—James E. Reeves.

Read by title at the request of the author.

9. *Note on a New Rotiferon, Gomphogaster areolatus.*—Charles M. Vorce.

Mr. W. H. Seaman moved that an engraving be made of the old microscope exhibited by Mr. C. C. Mellor for the Proceedings. Referred by rules to the Executive Committee.

Dr. W. P. Manton exhibited a mounting box and a camera.

Mr. Edward Pennock exhibited a simple form of centering substage. The centering is accomplished by means of two eccentrics working at right angles to each other.

Adjourned.

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THURSDAY P. M. September 1, 1887.

Working session.

The whole of Thursday P. M. was devoted to practical methods of work. The several small rooms of the chapel as well as the main room, were occupied by tables for the convenience of the demonstrators. The committee in charge of this feature consisted of Jacob D. Cox, Thomas J. Burrill and D. S. Kellicott.

(*a*) The application of the Solar Microscope.

(*b*) Illumination of the Microscope with Electric Light, using Gundlach's Apochromatic Objectives. By L. D. McIntosh.

1. Demonstration of the method of preparing Casts, cutting Sections and mounting them. By James E. Reeves, assisted by Mary A. Spink.

2. (*a*) Cutting Sections by the "dry" method.

(*b*) Cutting Sections of injected specimens by the "wet" method, and mounting them in Canada balsam. By R. N. Reynolds.

(*a*) Section Cutting with Laboratory Microtome of Bausch & Lomb Optical Co.

b) Use of New Freezing Attachment to the above Microtome. By W. A. E. Drescher.

(*a*) Cutting, staining and mounting Vegetable Sections.

(*b*) Permanently marking slides. By J. J. B. Hatfield.

An arrangement for erecting the image with Binocular Microscopes. By R. H. Ward.

General Mounting. By Frank F. Colwell.

Methods of detecting adulteration in food materials. By C. M. Vorce.

Demonstration of a method for preparing Areolar Tissue. By Simon H. Gage, assisted by E. H. Sargent.

Mounting Living Objects in Life-slides. By Jos. H. Logan.

Methods of producing the crystalization of Chemicals. By W. H. Seaman.

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Demonstration of the methods of embryological research ; Incubation ; opening of the egg ; hardening ; staining *in toto;* imbedding ; section cutting in the Thoma Microtome ; mounting. By W. P. Manton and C. G. Jennings.

The apparatus used in this demomstration is from the Microscope Laboratory.

Cutting, grinding and mounting Favosites Hamiltonia, Eozoon Canadense and Alicolites Davidsonia. By Chas. Wellington and Mrs. Wellington.

Sharpening section and other knives. A simple and cheap method of engraving. Mounting in glycerin. By F. L. James.

Methods of collar correction for immersion objectives. Use of Prof. H. L. Smith's Apertometer in measurement of balsam angle of objectives. (Visitors are invited to bring immersion objectives for measurement). By H. R. Spencer and E. K. Buttles.

Preparing Sponges. By Henry Mills.

FRIDAY A. M., September 2, 1887.

The session was called to order by the President.

Recommendations of the Executive Committee were first considered.

1. The following nominated and elected members of the Society: Dr. Augustine Rue Booth, Shreveport, La.; Dr. C. Q. Jackson, Pittsburgh, Pa.; Placide Reynes, New Orleans, La.; Wm. Shannon, Beaver Falls, Pa.; Mrs. J. C. Eddy, Cleveland, O.; Jas. H. Porte, Pittsburgh, Pa.; Edwin T. Painter, Pittsburgh, Pa.

2. The resolution relating to working session [see p. 351] was re-reported with the recommendation that it pass. On motion it was adopted.

3. By vote the proposition to give two bound sets of the Proceedings to the libraries of Pittsburgh was adopted. The Iron City Microscopical Society was asked to receive them and place them as they desired.

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The report of the Committee on Nominations of Officers was then presented:

President.....Prof. D. S. Kellicott, Buffalo, N. Y.
Vice-Presidents.....{ Prof. T. B. Stowell, Cortland, N. Y. / Prof. H. J. Detmers, Columbus, O.
Secretary.....Prof. T. J. Burrill, Champaign, Ill.
Treasurer.....Dr. S. M. Mosgrove, Urbana, O.
Executive Committee.....C. C. Mellor, Pittsburgh, Pa. / Dr. H. D. Kendall, Grand Rapids, Mich. / ..... Dr. R. J. Nunn, Savannah, Ga.

By vote Mr. C M. Vorce was directed to cast the ballot of the Society for the officers nominated. After the vote was cast the President declared the gentlemen named officers elect of the Society.

The reading of papers was resumed.

1. *The Fallacies of Bacteriological Research.*—George W. Lewis.

The author was absent and Dr. George E. Fell read the paper.

Dr. Fell, in discussing the paper, said he could not well dispense with the microscope in diagnosing Tuberculosis, for example, he gave an instance: Three cases were examined, the sputa of number one was found to contain Bacilli, in the other two he found none. The man from whom the first was derived has since died of phthisic; the other two are living.

Dr. Detmers said the method by culture was too slow; the patient might be under ground before the culture shows results. He quoted a case in which Koch had failed in 1865, owing to want of homogeneous objectives; we are much indebted to our opticians for our knowledge of bacteria.

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Prof. Burrill related an instance where Prof. S. A. Forbes had been cultivating insect destroying bacteria, until thoroughly familiar with their appearance, yet in some tubes, considered characteristic, saved for some time, on microscopical examination, showed a different organism. He believed in the microscope. Dr. Detmers had known similar cases.

Dr. R. J. Nunn urged the importance of immediate microscopical examination.

Dr. R. H. Ward, chairman, presented the report of the committee appointed last year on the Fasoldt plate.

The President said he had been a most interested listener and if Mr. Fasoldt had ruled the lines he should receive the credit he deserves, but the lines must be counted. He, continuing, said he could rule lines so close together that there would be no space between them; as Mr. Fasoldt's plates are ruled with the alternate lines longer they could be counted and photographed. Nobert submitted to this test triumphantly and Mr. Fasoldt must do the same, but he gave the committee notice that he should attack the report most vigorously. He wished to say again that Mr. Fasoldt deserves great credit for his rulings.

Dr. Detmers said he would attempt to photograph the lines; for if he could count them he could photograph them.

2. *The Life History of the Diatomaceæ*, Part II.—Hamilton L. Smith.

Read by title.

3. *A New Photo-micrographic Camera.*—George W. Rafter.

Read by title.

4. *History of my use of the Microscope.*—Ephriam Cutter.

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Read by title.

5. *The Cardiac Muscle Cells of Man and certain other Mammals.*—Boardman L. Oviatt.

The paper was read by Prof. Gage.

Dr. Fell and Prof. Seaman complimented the work shown in this paper.

President Rogers said he felt like complimenting Cornell University on the work shown at last year's meeting as well as this.

6. *Zeiss' Apochromatic Objectives.*—H. J. Detmers.

Prof. Gage said he had obtained three months ago a microscope and three objectives from Zeiss; he had tested them on histological work, but thought them no better than others in his possession, still they are most excellent in giving clear definition and ease to the eyes.

Dr. Fell said that this was of importance and Zeiss should have the credit for it. An objective should be thoroughly tested before judgment was rendered.

Mr. H. H. Turner thought that the superiority of the latest objectives was in the absence of color.

Prof. T. J. Burrill presented for a special committee the following, which were unanimously adopted:

Resolved, That the heartiest thanks of this Society are eminently due, and are hereby cordially tendered, to the local committee of the citizens of Pittsburgh, and especially to the worthy president and members of the Iron City Microscopical Society, for the warm reception given us and for the numerous attentions and favors received at their hands.

Resolved, That we offer many thanks to Carnegie Bros. & Co. for the opportunity of visiting their renowned steel works and the attention shown us there; to the Westinghouse Electric Co. for the exceptional and highly-prized privilege of inspecting their large and important

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works; to the Allegheny Observatory and Professor Very, who kindly guided our observations through the former; to John A. Brashear for a most interesting visit to his optical works; to the Engineers' Society of Western Pennsylvania for the generous tender of the use of their rooms; to the Chamber of Commerce for documents which will prove interesting souvenirs of our visit, and to the local Press for the interest shown in our work and for their careful reports of our proceedings.

Adjourned.

FRIDAY P. M., September 2, 1887.

President Rogers called the Society to order at 2 P. M.

The question of place of meeting next year was called up and after the views of members had been freely expressed, on motion the determination of time and place was left wholly to the Executive Committee elect.

Papers and Reports were read as follows:

Comparison of Fasoldt II, with Centimeter Scale "A."—Marshall D. Ewell.

Read by Prof. Rogers.

The Treasurer, Dr. Fell, presented his annual report, [see p. 326]. Referred to the Auditing Committee.

The same offered the following amendment to Article IX of the Constitution: "The entrance fee shall be two dollars and the annual dues shall be three dollars, paid in advance." The remainder of the Article to remain as at present. Referred to the Executive Committee.

The Erysipheæ of Illinois.—Thomas J. Burrill.

Notes on Microscopical Exhibitions.—R. H. Ward.

Read by title.

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Permanent Potassium Hydrate Preparations.—Boardman L. Oviatt.

Read by Simon H. Gage.

Boardman L. Oviatt offered in substance the following on Permanent Potassium Hydrate Preparations:

While using the method of obtaining ultimate elements of tissue held together by cell-cement given in his paper on Cardiac Muscle Cells [see p. 283], he found it possible to permanently preserve the mounts. The tissues having been macerated a sufficient length of time in the potassium hydrate solution are teased out on slide and the cover glass is put on. A saturated aqueous solution of potassium acetate is now run under to excess and allowed to stand half an hour. The liquid is then drawn away with filter paper, the specimen retained in place and glycerine run in, after which it only remains to seal in the ordinary way.

A similar method was published by Professor Gage in *The Microscope* in 1886 [p. 267].

Dr. Fell reported for the Committee on Standard Micrometer.

The report called out remarks by the President, in the course of which he agreed to make twelve copies for the Society free of charge, unless ruled on glass with parallel faces, in this case the glass should be paid for.

The Secretary reported the state of the slide collection.

The Committee appointed at Rochester on the Standard Micrometer Screw asked to be discharged from further consideration of the matter. Agreed to.

The amendment to the Constitution proposed at the Chautauqua meeting was reported on adversely by the Executive Committee. Report adopted by vote of the Society.

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President Rogers said the business of the meeting having been completed, the time had arrived for final adjournment. He desired to thank the Society for the courtesy and cordial support he had received. He asked Prof. Burrill and Dr. Fell to conduct the President-elect to the Chair which he would resign.

D. S. Kellicott, on taking the chair, thanked the Society for the confidence expressed by election to the high office of President of this Society; it is an organization he believed in and believed it had a field and a future. He would endeavor to serve the Society in the new relation with the same faithfulness that he had served it for six years past as Secretary, *i. e.*, to the best of his ability and opportunity.

By resolution the formal thanks of the Society was tendered to the retiring officers.

On motion of Prof. Rogers the meeting adjourned without day.

D. S. KELLICOTT,
Secretary.

S. M. MOSGROVE,
Asst. Secretary.

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